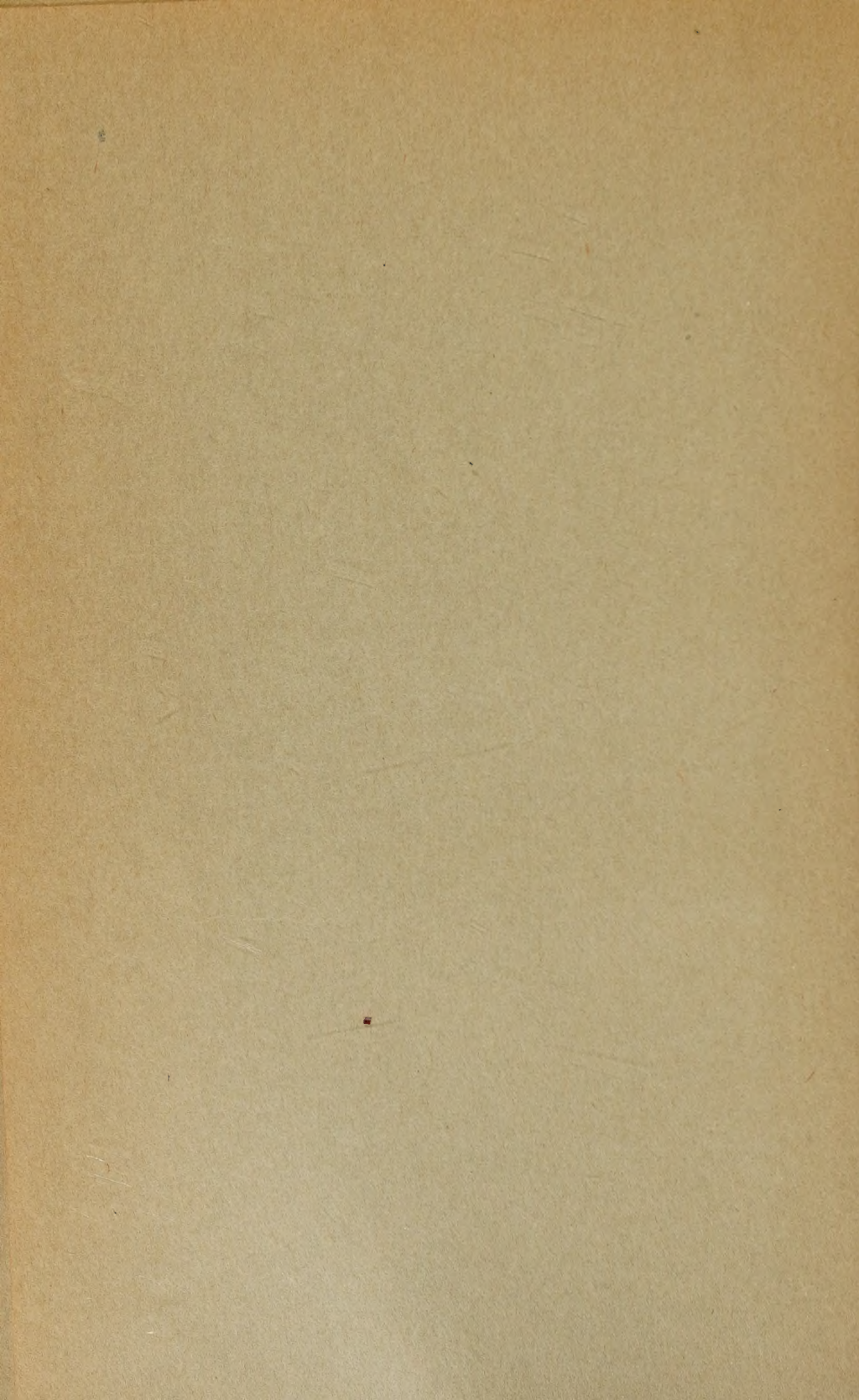


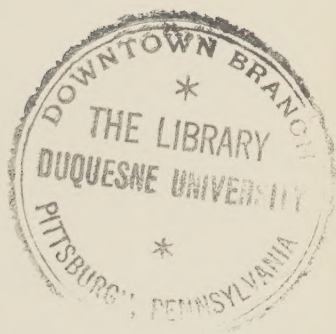
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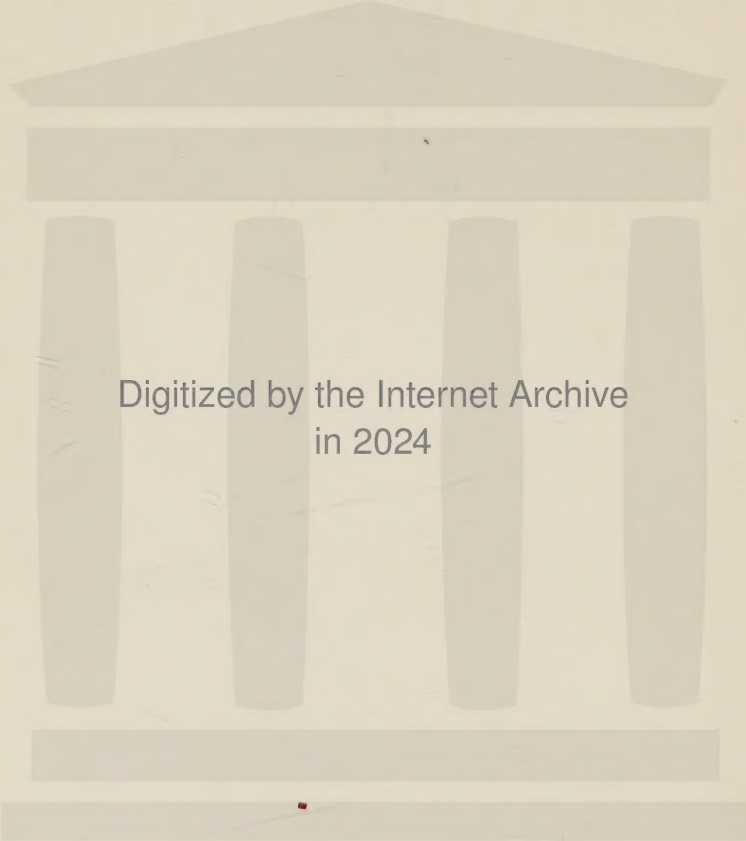
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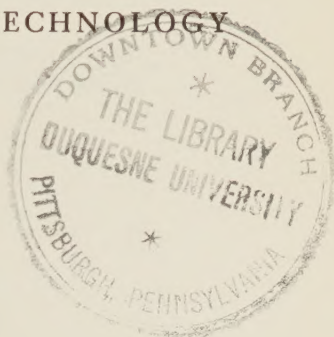
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Horizons & Problems



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Foreword

The Graduate School of Industrial Administration, William Larimer Mellon, Founder, at Carnegie Institute of Technology was dedicated in ceremonies held on October 2 and 3, 1952.

The School's new building was formally presented to Carnegie at an evening meeting on October 2 at which Mr. Sidney A. Swensrud, President, Gulf Oil Corporation, spoke on the subject: "What Does Industry Expect?"

The following day, twelve of the nation's leading business administrators, graduate educators, and researchers participated with representatives of Carnegie in a closed round-table discussion of the general subject of fundamental research in administration.

In order to make this discussion available to other university administrators and researchers, and to other business leaders concerned with research in business administration, this book has been published. It includes both Mr. Swensrud's remarks and a condensed record of the round-table discussion. We hope that publication of this assessment of the current state of research in administration and of promising future paths for research will help to promote better understanding and research of lasting value for American

FOREWORD

business and the American public. Thanks are due to the Carnegie Corporation of New York for making the publication financially possible.

Professor Melvin Anshen of the Graduate School faculty has been largely responsible for preparing this volume for publication.

Establishment and Objectives

The Graduate School of Industrial Administration was established at Carnegie in 1949 through a gift of six million dollars from the W. L. and May T. Mellon Foundation. The purpose of the gift was to provide an opportunity for young men of demonstrated promise to lay a sound educational foundation for future growth to positions of responsibility in private industry and public service.

The present way of life in America rests heavily on the contributions American business leaders and engineers have made during many decades, working as part of a free, integrated society. The future of this way of life will depend in large part on the men who rise in industry during the years ahead and on their capacities for efficient operations and broad-gauged leadership. It is men of these ultimate potentialities that the School seeks to train.

The aim of the School is to select students of outstanding promise, and to help them build a foundation of fundamental knowledge, disciplined mind, and character for future growth in industry, in civic responsibilities, and in personal life. Special emphasis is placed on preparation for work in industrial and business establishments where sound training in both engineering and management is required.

Participants in the Round Table

INDUSTRY

JOHN L. COLLYER, President, *B. F. Goodrich Company*

LELAND HAZARD, Vice-President, *Pittsburgh Plate Glass Company*

ERIC L. KOHLER, Management Consultant

GWILYM A. PRICE, President, *Westinghouse Electric Corporation*

JAMES H. ROBINS, President, *American Pulley Company*

SIDNEY A. SWENSRUD, President, *Gulf Oil Corporation*

FOUNDATIONS

JOSEPH M. MCDANIEL, JR., Assistant to the Director, *The Ford Foundation*

OTHER EDUCATIONAL INSTITUTIONS

EDWARD P. BROOKS, Dean, School of Industrial Management, *Massachusetts Institute of Technology*

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geles*

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Professor of Industrial Admin-
istration and Psychology

HERBERT A. SIMON, Head of De-
partment of Industrial Man-
agement

ELLIOTT DUNLAP SMITH, Provost

DEDICATORY ADDRESS



What Does Industry Expect?

By

SIDNEY A. SWENSRUD

President, Gulf Oil Corporation



WHEN business invests money in a new enterprise it usually is the result of some hard thinking about objectives and how to achieve them. And business usually has clear expectations about results.

William Larimer Mellon, in whose name this new School has been so generously and firmly founded, was a man of vision and imagination—and he was a business man. I know that before this investment was made there was hard thinking about objectives and there were expectations as to results. And since, in a very direct way, this investment has been made by the Mellon Foundation in the interests of all American industry, I think it is important right at the beginning to raise the question: What does industry expect of this new Graduate School of Industrial Administration?

If William Larimer Mellon were here this evening to express his views on this question, I think I know some of the things he might say. They would be direct and simple things, for W. L. Mellon was a direct and simple man, with an eye to those few central fundamental qualities which are the core of every problem. He would be concerned not only with the service of this graduate school to American industry, but equally with the School's capacity for training

men of wide interests and deep civic responsibility. For these are a vital part of business leadership in our American society.

I believe industry should expect three things from this new graduate school—the same three things I suspect Mr. Mellon may have hoped for.

First, men soundly and fundamentally trained to assume leadership in American industry in the years ahead.

Second, men broadly trained to provide civic leadership in maintaining the kind of free society America has traditionally cherished.

Third, fundamental research that will help American industry and its management become ever more efficient, for here is the foundation of continuing increases in the entire nation's standard of living.

Let me try to spell out what these three goals mean to me.

TRAINING FOR MANAGEMENT

WILLIAM LARIMER MELLON's lifetime saw a managerial revolution. The world into which he was born in 1868 was a world of gas lights, of small businesses, of simple factories and industrial processes. In one man's life span, all this has changed. Modern industry is vast and complex. No one man can hope to know and manage in detail all the elements of any major business concern of today. Modern power and modern processes and techniques have revolutionized industrial processes, and have taken them beyond the comprehension of any but the expert technicians. No man can hope to provide the capital for even a small part of today's industrial giants—unlike the owner-operator of yesterday who put up his own funds, established and managed his own

business, knew its innermost details, and pocketed its profits—or faced bankruptcy through its losses.

From this vast complex of change, I would single out two developments of major significance for this new graduate school.

First, the day of the professional manager is here. Increasingly, the nation's leading business concerns are managed by men who have made business their profession. Their task, if you please, is to manage the experts—to pull together the vast aggregations of men, money, and materials that make up modern industry, welding them into an efficiently operating whole. Even the wealthy among them cannot provide more than a small share of the resources for their businesses; it is their job to manage the investments of others. Most important, it is their job to organize, to mesh together the thousands of production resources of our society so as to produce the goods and services that are wanted by 160 million Americans and by millions in other countries as well.

Second, modern management rests increasingly on fitting together men and technology. True, it is seldom necessary for the manager to know the details of the technology on which his industry rests. But unless he is reasonably at home with it and unless he understands how the engineers and scientists in his organization think, he labors at a real disadvantage. In many fields in American industry today, an increasing number of management jobs are being filled by men who were engineers by original training. Yet every management man knows that it is all too easy for the engineer and the scientist to be narrowly trained and oriented, unsuited for taking on management responsibilities because of his lack of broad knowledge and understanding of the many other phases of business. Knowledge of technology is

important in modern industry, but the engineer alone is apt to be a mere technician.

The engineer who is going to be more than a skilled technician has to broaden his training and his understanding in at least three important ways.

One, he has to understand the meshing of functions in a business enterprise. He has to know the inter-relations of production, marketing, finance, human relations; the problems raised by these relationships; and the ways in which the professional manager endeavors to deal with these problems.

Two, he has to understand the limitations of the strictly technical approach to problems and to appreciate how, in favorable circumstances, imagination may break through these limitations. In the opening pages of his stimulating analysis of American Capitalism, Dr. J. K. Galbraith reminds us that the aerodynamics and wing-loading of the bumblebee demonstrate that, in principle, it cannot fly. And yet, every day, the bee defies the august authority of Isaac Newton and Orville Wright.

Three, he has to understand that the management of business enterprise is not only the organization of materials, equipment, and processes. Even more important, it is the organization of human beings in effective working groups. How to get people to do things is the prime question in every discussion of labor relations, in every discussion of the administration of salesmen, in every discussion of supplier-customer relations, in just about every discussion of organization and management.

If this assessment of modern management is correct, W. L. Mellon's vision was sound when he founded this new graduate school of industrial administration. For it is specifically aimed at taking carefully selected men with engi-

neering backgrounds and training them—thoroughly and fundamentally—for modern industrial and business management.

Industry needs men trained in this way. We in industry expect the graduate schools to lay the foundations for management growth. We expect men trained to understand thoroughly the few fundamentals in each field of management responsibility. We expect men trained to size up each problem in an orderly but imaginative way. We expect men trained to keep their eyes and ears open, to learn from everything they do and everything that goes on around them. We expect men trained to hold uppermost their professional and personal responsibility and integrity. Management is a new profession. The graduate schools can do much in building into young men looking toward management the ideals and visions that have marked the pioneers in every other profession.

There is a negative side to these suggestions. Routine techniques and skills are relatively easy to obtain in modern industry, even when manpower is as tight as it is today. The man trained in the details of accounting, or operation, or marketing plays an important and useful role. But industry can train for these skills. I doubt that a graduate school of industrial management aimed at developing selected men of top-notch quality should stress them too much. It is the broader-gauged man who is scarce—the man who sees beyond today's job, the man who can take on the new and different task, the man who knows his fundamentals well and learns the details as he needs them, in short, the man who is the professional manager of tomorrow. These are the hard qualities to find. They are the hard qualities to develop. I don't know whether this graduate school can find or develop them. But it is where I would set my sights.

TRAINING FOR CIVIC LEADERSHIP

MANAGEMENT's first job is to make profits. This is not only because management is employed by the owners of the business. It is equally because this is the way management and the capital it manages can make the greatest contribution to the common welfare. Adam Smith, far back in 1776, first spelled out the profit mechanism. "The search for profits through open and honest competition," he said, "gets the most goods produced for the greatest number." This is the basic premise on which the private enterprise economic system rests. Adam Smith's "enlightened self interest" is the gas in the economic engine today, just as it was one hundred and seventy-five years ago. Every man looking toward management as a profession should be proud to be digging hard for profits, not ashamed of them as some shallow critics of the American system today would suggest.

But management's job does not stop at the factory walls. Today, as never before, the basic premises of the American free enterprise economic system are being questioned. Every man aiming for management in America today needs a vision—a vision of an alert American industry. He must recognize that industry will prosper only if it satisfies the fundamental needs of the American people. Yet he must stand steadfast for those fundamental principles of individual freedom and human dignity that brook no compromise. The graduate schools have no harder job than this—to develop a balance in the managers of tomorrow between adaptation to a rapidly changing society on the one hand, and firm adherence to basic moral and economic principles on the other. Only through such a balance can American industry—indeed, all of American society—survive free and sound.

If the private enterprise economic system is to survive in

America, it will not be because we in this room and others in management circles want it. It will be because the private enterprise system is what the great majority of the American people want—laborers, farmers, white collar workers, all 160 million of us. American management's first job is to run the nation's businesses well—a smoothly functioning economy is the best advertisement for private enterprise. But management must also take leadership in convincing the American public that this *is* the best kind of economic system. "Speak for yourself, John!" was sound advice, and it still is.

The men who come into management must understand the whole sweep of modern economic, political and social life. They must sense how things look to "the other fellow," for in a democratic political system the other fellows have the votes that give them vast power over American industry in the showdown. They must understand the basic philosophy of the private enterprise tradition, and be willing to stack it up honestly and openly against any competing system.

Perhaps there was a day when management could tell the rest of the country—Take it and like it! But if there ever was, that day is gone. Today we need from the graduate schools a group of men who are willing to go beyond the offices of management out into the factories, and beyond the factory walls out into their communities—who will play a role of leadership in community affairs that is cooperative and honest, and who will sell the merits of private enterprise and American business both by what they do and what they say. We need men who can face the issues, with real understanding. Dogmatism and desk-pounding will not convince many doubters. Private enterprise needs to put its best foot forward. The young men at the operating levels in in-

dustry can do a lot, perhaps more than we can do from the front office.

THE ROLE OF RESEARCH

I AM a corporation president, not a research scientist. But from my long experience in the oil industry, I have formed a solid faith in the importance of research. American industry today rests on the accumulated knowledge of hundreds of years of research, much of it now taken for granted in our everyday lives—electricity, aluminum, airplanes, radio.

But the research that led to many of these inventions was esoteric, far-fetched, “long-haired” in its day. Scientists and researchers often fought an uphill battle to get their ideas accepted. In this cumulative sweep of research American industry has played a proud role. It has encouraged and financed many of the men and methods that pioneered the way. But we must admit in honesty that much of the fundamental work that underlies these industrial applications was done in university laboratories—often on a shoestring, with inadequate equipment, and by men who could have earned much more by giving up their research careers for more immediately practical work in industry or government. We have gained more from the scientific research of our universities than most of us know.

Business administration—indeed, the whole study of human behavior on a scientific basis—is a new profession. I make no apologies for American management. I think we have done a good job. But I suspect we may have much to gain from the development of basic research in administration, comparable to the basic scientific research that has underlain the phenomenal development of modern technology over the past century.

HORIZONS AND PROBLEMS

Business administration as a profession is growing rapidly. Much research has been built up over the last three decades. But this is a tiny period in the long history of business. And much of the new research has been aimed at short run, immediately practical problems. Such research has generally sought to report what are the best prevailing practices in industry, seldom to search for new methods nowhere used or even considered.

It may be that management today is in many respects where medicine was a century ago—a field of intelligent, alert practitioners operating largely by tradition and accumulated experience, but with little agreement on what is truly fundamental principle. Many sick men were healed a hundred years ago. And with the development of outstanding medical schools and professional medical standards doctors' contribution to the public welfare steadily increased.

But an honest evaluation of history would report, I think, that the basic change in medicine over the past century has come mainly from the research laboratories, not from the classrooms or the practicing doctors' offices. If you and I live far longer than our parents, it will be largely because of the sulfa drugs, penicillin, streptomycin, dozens of other drugs, vaccines and inoculations, and vastly improved knowledge of the workings of the human body that permit today's doctor to perform feats undreamed of only two or three decades ago. Biophysics, chemistry, biology—these and their fellows have been the real but often unsung heroes.

Perhaps there is comparable basic research into the fundamentals of human behavior that can pay equal dividends in permitting human beings to work together more effectively. Certainly there is much we need to know about why our organizations work well under some conditions, and badly under others. The mere problem of communication be-

tween top management and the man on the production line has frustrated more good management intentions than imagination can conceive—unless you have experienced it firsthand. On the technological side, the merger of managerial and technological advances in the “automatic factory” has made surprising strides in some of the continuous process industries; research can push along faster these contributions to the American living standard.

I hope this graduate school, and others as well, will live with a research vision—both to be immediately useful to industry, and to do the fundamental probing and digging that looks to the decades ahead. Industry must be primarily concerned with getting its jobs done. We have to survive from day to day, or, I might say, from crisis to crisis! As in medicine, science, and engineering, the universities have the time and objectivity to view us and our problems as we can hardly view ourselves.

We in industry have a right to expect a sympathetic, understanding approach. But we must realize that some of this research may be slow and impractical in appearance—that it may look as long-haired and queer as atomic physics and penicillin molds did only a few years ago. We need to encourage basic research—to cooperate whenever we can, since industry is the prime laboratory for research into management. And we need to be ready to face up to the fact that results may be temporarily disruptive to long established patterns of business operation. They may be disturbing to the men who have grown up in the old ways. Progress is seldom painless! I hope all of us, faced with the disturbances introduced by new ideas, will have the courage and the self-knowledge of that Chinese statesman of long ago who, also concerned with the disturbance of change,

HORIZONS AND PROBLEMS

prayed, "O God, revitalize China, and O God, begin with me."

CONCLUSION

BEFORE writing this talk, I reread the announcement of the grant from the W. L. and May T. Mellon Foundation founding this graduate school of industrial administration. Perhaps if W. L. Mellon were here this evening, his vision of the School's future would be different from mine. But in the announcement is this same three-fold emphasis—sound graduate education for management, broad-gauged responsibility for citizenship, and support for fundamental research. It is a far-seeing document, with a sharp focus on the management problems of today merged with a vision of more effective industrial management for the future. It marked a gift worthy of the great Mellon family tradition of service to the Pittsburgh community, to American industry, and to the nation. May Carnegie, her administration, and her faculty make the fullest use of it in this same tradition.

THE ROUND TABLE



Fundamental Research
in Administration:
Horizons and Problems



Prior to the discussion, the following memorandum was circulated to the participants.

The Development and Current Status of Research in Administration

INDUSTRY'S debt to research in the natural sciences is universally recognized. Physics and chemistry, in particular, have revolutionized manufacturing and processing industries in the last hundred years. Research in administration itself and in the social sciences that underlie it has not had anything like the same significance for industry. Executives direct human beings in human institutions with little formal assistance from research. Systematic research has had comparatively little to contribute to the process of making and executing decisions, which is management's principal responsibility.

This circumstance suggests two questions:

1. Does this disappointing record reflect real weaknesses in the research and its application to the problems of business management?
2. Alternatively, does the record reflect a failure to understand how the growth of knowledge in administration, economics, psychology, and the other social sciences can contribute to management's performance of its job?

Round-table discussion may clarify these issues and help to find ways in which fundamental research can be brought to bear on the central problems of managing business organizations. It can help to discover how the professional schools can pursue such research along lines that will be of maximum long-range value to American business and its management.

RESEARCH IN ADMINISTRATION AND ORGANIZATION—THE HISTORICAL RECORD

RESEARCH in administration had its beginnings in the work of Frederick Taylor and the other "fathers" of the scientific management movement. They made a number of important discoveries—most of them relating to increasing the efficiency of the individual worker. Breaking the job down to its component parts, time and motion study, mechanization, incentive compensation—these are some of the techniques that contributed to gains in efficiency and productivity. A contribution of even greater general importance was Frederick Taylor's belief that the methods of scientific research as they have been developed in physics and chemistry could be applied to problems focusing on the worker, his materials, and his tools.

A new stage in administrative research began twenty-five years ago with the controlled experiments conducted at the Hawthorne plant of Western Electric Company by a research team from the company and the Harvard Business School working under the inspiration and guidance of Elton Mayo. The research pointed up the great importance of factors other than compensation and the physical conditions of work in lifting productivity. It opened the door to a new area for investigation. The motives, hopes, and social morale of individuals and groups became a focal point for administrative research and introduced the era of "human relations."

RESEARCH IN ADMINISTRATION AND ORGANIZATION TODAY

BOTH the scientific management and the human relations traditions are represented in current research activities. The former is found principally in the field of industrial engineering, the latter in social and industrial psychology, within and outside

the business schools. Three general observations can be made about these two main strands of research.

(1) Both avenues of research have concentrated on the individual worker, on groups of workers operating as teams, and on the lower levels of supervision. The range of knowledge in these areas is steadily increasing. There has been less disposition, however, to extend this type of research to middle and especially top management levels.

(2) Over the past two decades, the leading business schools and professional associations like the American Management Association have extensively surveyed and reported administrative and organizational practices followed by leading firms. This type of survey research has a valuable role to play, but it does not meet head on the critical problem of developing better practices than the existing best.

(3) Relatively little progress has been made in (a) developing scientific techniques for testing and validating existing practices, and (b) discovering and testing new ideas and methods, previously unknown. Great new possibilities for research may be found in these areas.

The distinction made in (2) and (3) between surveying existing practices on the one hand, and testing existing and new ideas through research on the other, may be clarified by an example drawn from staff and line relations in industry. Every executive with a large organization faces problems in this area. What has research contributed, and what can it contribute to practical solutions of these problems? There is a standard chapter in almost every book on industrial organization that describes three forms of organization—usually called line, functional, and staff and line. After listing these, certain advantages and disadvantages of each are presented—and the chapter ends. The claims made for each form are apparently based upon the accumulated experience of business concerns, but systematic surveys of this experience and careful research designed to test

FUNDAMENTAL RESEARCH IN ADMINISTRATION

the claims for each approach under varying conditions are not available to support the conclusions. It may be that common sense and our accumulated, unanalyzed experience tell the executive everything he needs to know about staff-line relations. But he does not *know* these things in the sense that a metallurgist *knows* how to make a piece of steel of particular specifications.

Increasingly, thoughtful observers are questioning whether the practical knowledge we now have about administration cannot be strengthened by the kind of research efforts that have proved so successful in the technological area.

SOME RESEARCH QUESTIONS FOR CONSIDERATION

CURRENT research along the lines of (1) and (2) above is well under way, although it can gain much from careful reassessment and suggestions for improvement. The problems raised by paragraph (3) have been much less explored, and perhaps merit more attention by the round table. Here are some issues that might be considered:

1. What are some of the central problems faced by top and middle management? Some areas that suggest themselves are: the problem of effective organization structure; the relations between formal organization structure and actual organizational behavior; executive development; the making of decisions with incomplete information; the problem-solving process in executive decision-making; and the role and importance of face-to-face negotiation in labor relations. Which of these topics are "researchable," and which are beyond the grasp of existing research techniques? Are these the important topics? What are some of the other crucial areas?
2. How can industry and the universities cooperate in research on administration and organizational behavior?

What is the substitute, in this field, for the controlled experiment of the natural sciences? What techniques of observation and experimentation, as yet untried, could be employed without disrupting the operation of business concerns? What are the fruitful ways, for example, for discovering what is really involved in making an important policy decision in a business and in carrying it into effect through the firm's organization? Would it be useful to undertake detailed studies of the decision making and decision implementing processes? How can we evaluate the results of different processes?

3. Is the capacity of organizations to accept novel ideas and fundamental change less in the area of administration and organization than in the area of technology and the physical sciences? If so, why is this? Is the vital process of innovation and change in administrative organizations itself a "researchable" topic that needs to be investigated? What attitudes toward administrative research and the supplementation of judgment and common sense by scientific method ought to be developed among business executives? Business school students? Does the responsibility of the universities extend beyond their students on this score?

Introductory Remarks

MR. BACH: Speaking at our formal dedication last night, Mr. Swensrud expressed a judgment—and a hope—that lies at the root of the topic for this round-table discussion today. He said: "In management, we may have much to gain from the development of basic research in administration, comparable to the basic scientific research that has underlain the phenomenal development of modern technology over the past century." Mr. Swensrud went on to contrast the contributions of chemistry and physics with those of the social sciences.

I should like to add two or three personal observations on this subject. In the last few years I have talked to a lot of people in business and in the schools about this activity we call research. I was impressed from the outset by the difference between fundamental research attitudes in the field of administration on the one hand and the physical and natural sciences on the other. Research in administration has been carried on largely in terms of going out and looking at existing practices, describing them, finding out what works well in what circumstances, and reporting back on the "best" existing practices. I do not mean to suggest that this is true of *all* research in the field of administration, but I think it may be true of most of it. We report what is done, what seems to work well, and we add some suggestions about how to do it a little bit better. This contrasts with the physical and natural sciences where the attitude is quite different. There the primary objective of research is the discovery of new knowledge, the exploration of techniques that have never been tried before.

Another difference made an impression on me. That is

the persistent inquiry of the physical or natural scientist: Why does it work this way? Why? As I visited around I formed the impression that this inquiry seems to be significantly less common among researchers in the field of business. Business people want something that will work, that will get the job done. I think this is quite natural because, as Mr. Swensrud was saying last night, they have to get their work done. The drive to advance knowledge by giant strides, although by no means absent in the field of administration, seems to be notably weaker.

A third observation is that business is not making very effective use of the research that the schools are carrying forward. This contrasts with the highly effective use that the companies managed by several of you gentlemen are making of research in the physical sciences. Companies like Goodrich, Gulf, and Westinghouse are built on technological research. It may be that we in the academic world ought to be doing more than we are doing, or that we're not doing the right things, or, perhaps, that we are doing the right things but are suffering from a lack of effective communication with business managers.

The main point in calling this conference is this. We have been sufficiently impressed by these observations to think it important to get together a group of men of your distinction and experience to sit down and talk very informally about these problems, and to examine the areas that seem to top management to be the most important for the schools of business to be researching about. I can assure you that this is a matter of deep concern not only to us here at Carnegie, but to every responsible academic research organization.

These observations suggest a whole series of issues for exploration. Some of you, at the outset, may be unwilling to accept this appraisal. If you are willing to accept it, at

least as a point of departure, then you may want to ask whether the circumstance described by Mr. Swensrud is the result of a failure by researchers to ask the right questions, or a failure to understand how to apply research techniques to the complex problems encountered in the management of business organizations. Or perhaps there are inherent difficulties, still to be explored, in carrying on basic research in the management field. It may be—I know it has been suggested—that we are still mired in some elementary confusions about words and their meanings. I've had trouble myself, in talking to business executives about some of the research currently in progress here at Carnegie, because my concept of "research" was different from the business man's.

Plato tells us that Socrates, who long ago carried the device of round-table discussion to a higher level than even our most gifted contemporaries, was fond of warning the participants: "If you want to talk with me, first define your terms." I suggest that we open our discussion by heeding that warning. I have asked Professor Simon to describe for you a research project on which several members of our faculty have been engaged recently, by way of illustrating one operational meaning of such terms as "research" and "research methods."

The Research Approach to Management Problems

THE CONTROLLERSHIP STUDY—AN ILLUSTRATION OF ONE RESEARCH APPROACH

MR. SIMON: Research has one thing, at least, in common with management. It is probably easier to do than to describe how it is done. The best way I know to describe in concrete, specific terms what research means is to outline briefly the steps that were involved in carrying out a piece of research on a problem of business organization.

During the past two years, we have been carrying forward a study of a problem in the organization of large business enterprises. We were concerned with the question, a puzzling one in many companies, of how far an accounting department ought to be decentralized to do a good job in a large, multi-plant company.

This problem seemed interesting and significant for a number of reasons. One was the immediate usefulness of the findings for resolving specific organizational problems. A second was that this specific question of decentralization is typical of a much broader class of problems that are of concern to top management—questions of staff-line relationships (relationships of an accounting department to the manufacturing and sales departments); questions of management incentives (the impact of accounting standards on executives); and questions of communications (the meaningfulness and significance of accounting information for operating executives).

MR. ROBINS: I don't know, Mr. Chairman, whether your preference is to operate with a loose or a tight rein over the participants. Will you let me interpose a question? Before Professor Simon plunges into a description of his project, will he pause to make clear why he thinks his study should be offered as an illustration of fundamental research? I have the feeling that he is describing the type of job that is not uncommonly handled by so-called management engineers. Does it represent research in the true sense? Why was it judged to be a suitable project for an educational institution? Just for the sake of smoking him out, let me suggest that it doesn't strike me as an assignment of the greatest usefulness for the talent that you people have.

MR. PRICE: I have that same general feeling, Dean Bach. You and Professor Simon have lunch several times a year with a few of your friends here in Pittsburgh, and in describing this project one day Professor Simon referred to it as fundamental research. I'm not sure that I can give you a formal definition of fundamental research as we use the term in our company, but we talk about fundamental research in the physical sciences, and we try to devote forty cents out of every dollar budgeted for research to that kind of thing. We think of it as the search for new knowledge that doesn't have anything to do with our business. We identify a second category as basic research, which we regard as the search for new knowledge in fields related to our business. We put fifty cents of our research dollar into that category. The last ten cents goes to support applied research. This is research of a practical nature focused on a single product, and we try to push this kind of research down to the plant level. Now, can you educators give us some examples in the field of business research that might be fundamental in the sense that we use the term in the physi-

cal sciences? Can you draw this distinction between fundamental or basic research on one side, and applied research on the other? To turn the question to Professor Simon, since between us Mr. Robins and I have taken the floor away from him, let me repeat our question: What do you regard as the fundamental research characteristic of your controllership project?

MR. SIMON: I'm glad you raised these questions. They go to the heart of the issues we want to talk about. The first point I want to make is that it was the broader implications of the study that marked it as a particularly promising subject for research. The question of whether a company should install a centralized IBM tabulating setup for handling payroll accounting is a common operating problem that is handed over to technicians in the accounting department for solution. At the technical level of procedures design it is not really a research problem, at least not the sort a university is well equipped to handle. A problem becomes a suitable subject for fundamental research when we become concerned with its ramifications—in this illustration, with the direct effect of the tabulating section on the accounting department organization structure; the resulting effect on accounting relations with operating departments; and the final effect on the use of accounting information by operating executives. At this point, too, it becomes a problem of sufficient scope to concern top management.

This is part of the distinctive flavor of fundamental research as we view it—that it seeks to investigate the specific problem as an instance of a broader problem; that it is concerned not only with immediate impact, but with longer and more obscure, but no less important, chains of consequences; that it draws on existing, organized bodies of knowledge—in this case knowledge about status, about incentives, and

about communications—to illuminate the specific problem.

The other part of the “flavor” of fundamental research is its method. If the problem we are studying is of sufficiently broad importance, we are justified in bringing to bear upon it a more intensive and more costly problem-solving procedure than the common-sense ones we use in dealing with specific everyday operating problems. Relieved of the necessity for reaching a decision immediately, we can explore in a careful, tentative fashion. We can formulate hypotheses and test them rigorously. We can devise new methods for obtaining data, and new methods for evaluating the data we have gathered. As a result, a close relationship develops between the research problem and the research method. The problem must be both broad and deep to justify bringing this heavy artillery to bear upon it. The questions that can be answered by a careful application of common sense are more quickly and cheaply answered that way. However great their immediate administrative importance, they are not research questions.

I don't know whether this reply meets your question fairly, Mr. Price, or yours, Mr. Robins. I think that if I carry the description of the controllership project a little further, it will give concrete meaning to what I am trying to say.

MR. PRICE: I'm not certain yet that a project of this kind should be described as “fundamental,” but perhaps the description will clear up some of my questions.

MR. SIMON: The accounting study involved five major activities. Let me elaborate just a bit on what was involved in each of these five steps. The first was to acquire know-how about the current operations of accounting organizations. We talked to controllers and other executives to find out what they thought the big organizational problems were and how they went about solving them in their

own companies. We also tried to explore what was known about staff-line relations, and how this knowledge applied to the question of accounting organization.

The second step was to sharpen up our criteria for judging whether an accounting organization was effective or not, or how effective it was. This was a task of determining how we could actually observe and evaluate outcomes. How could we assess the relative money cost of trying to do the job with one organizational structure as against another? What evidence would indicate objectively the relative effectiveness of the accounting service that was provided to management by one form of organization as against another? How, under different organizational arrangements, was accounting information actually used by management and operating personnel? Was there any way of identifying the long-term effects of the organization structure of the accounting department upon the development of executive personnel?

With some notion of the criteria we were going to apply, we were ready to go on to the third step of making sure that we knew what we meant by centralization and decentralization and that we had a proper framework for analysis. When we tried to decide how we would observe whether a department was centralized or decentralized, we found that this was not one question, but at least four. You can provide for a more detailed breakdown of accounting information. You can decentralize the geographical location of your accounting units by shifting them from the company offices to plant locations or to district sales offices. You can decentralize authority by putting the factory controller under the factory manager. You can decentralize communications by putting cost analysts down in the factory departments to work right with the department superintendents. There are

other aspects of decentralization, but these will serve as illustrations. Our later observations showed that these were four distinct questions, and that the answer to one of them didn't necessarily give the answers to the others.

In the fourth stage—the evidence gathering stage—we had to observe the effects, in terms of the agreed-on criteria, of these different organizational forms. We could easily enough have asked executives which arrangements worked well and which did not. But we did not want merely to record existing beliefs about organization structure. We wanted to assist management by developing new evidence that would not ordinarily be available to the executive from his own organizational experience. Unless we could add substantially to the existing body of experience and knowledge, we could hardly regard our research investment as profitable.

In our study we spent about half our time learning from accounting executives and controllers about their set-ups and procedures. We devoted the other half of our effort to interviewing the operating executives in these companies in order to find out how they actually used accounting information and what impact it had, what role it played in their decisions—how, in other words, the accounting service helped them in the actual operation of the business.

MR. KRAMER: In the business publication field, interviews with executives are, of course, one of our important sources of information about trends in American business. There are some real difficulties in getting good information on administrative practices. It's easy enough to learn about new developments in marketing, or production, or finance, but it's much harder to put a finger on trends in administration. How did you do the interviewing in this study?

MR. SIMON: We didn't ask operating executives directly about the accounting organization. As far as the vice-president for production and the vice-president for sales are concerned, that's the controller's problem. The operating man doesn't feel that he is the cook so far as the accounting setup is concerned. He is the customer who eats the soup. He can tell you whether he likes it, whether there is too much salt in it or too little, and how it is served. He can actually describe what kinds of accounting information he uses and how he uses it in making decisions and managing his subordinates; how he keeps in touch with the controller's department and they with him.

As you gentlemen can guess, sooner or later in one of these interviews the operating man would pull out of his desk drawer his little "black books"—the well-thumbed handwritten records that he or his clerk had transcribed for his own use from accounting or more often from his own operating sources. These black books gave us valuable clues as to his understanding and use (or non-use) of the accounting system, the ways in which the operating man liked to have information reported, the kinds of items that related most directly to his daily operating problems and that he wanted to have immediately available.

This is an example of the kinds of basic data—the raw material for analysis—we were looking for. The important point at this step is that the executive tells the researcher what he does or, even better, lets the research man observe him in action. From these reports and observations of actual behavior, and from knowledge of the findings of other studies of organizations, the research team has to draw its conclusions.

For example, previous research on organizations going all the way back to the famous Hawthorne studies has repeat-

edly revealed that effective communication between two individuals in an organization is not likely to develop or be maintained unless their actual work assignments bring them into frequent contact. This generalization suggested to us certain questions that should be asked of operating men and accounting personnel about their contacts and led to a confirmation of the general proposition as applied to this particular problem.

The fourth stage of the inquiry leads directly into the fifth: the interpretation and evaluation of the findings. We were interested not only in what could be learned about accounting organizations, but also in adding what we could to the general body of knowledge about organizations and their behavior. Since the controller's department is just one example of a staff department, the conclusions reached here—for example, with respect to the division of authority over the factory controller—presumably also have significance when generalized to apply to a personnel department or an industrial engineering department. One of the characteristics of the research is that it attaches quite as much importance to these generalizations as it does to the more specific conclusions; although not, I hope, to the neglect of the latter.

May I say that I have been enjoying the novel experience here of having some of my work described as too "applied"! This has seldom happened to me. In my own work, I have really not been able to draw a sharp line between the concepts of fundamental and applied research. I have never believed that if a thing is fundamental it therefore is not practical—although certainly the inference is correct that usually something that's fundamental doesn't get applied for quite a while and doesn't get applied directly. In the field of business research, however, I think we ought to be prepared

for a lot of by-product applications that are rather direct and immediate, flowing from research that can fairly be called fundamental.

I tried to indicate earlier what I thought was fundamental about the controllership research. It's the direction you take that counts. Does the research take you toward generalizations? Does it relate to other fields of knowledge? In our study, for example, one of the things that was of the greatest interest to us was the observation that there appeared to be a barrier against communication between accounting and manufacturing. This seemed to result from the fact that when you looked at the kinds of numbers the accountant was writing down in his records you found they didn't fit the "equations" the plant engineer was using to solve his blast furnace problems. The two men had different frames of reference and the information provided by the accountant didn't fit into the thinking system that was directing the blast furnace. It didn't show the engineer what valves to turn, or even give him a notion of how to relate the information to his own problems. This observation has brought us to a serious re-thinking of the whole psychology of the problem solving process. And we have some tentative plans for doing a fair amount of research on this issue as it applies to administration. This, I suppose, is a better example than the one I gave earlier of what we think is fundamental about the project.

MR. COLLYER: Professor Simon, I wonder if you would summarize, by way of example, some of the more important organizational conclusions reached as a result of your study?

MR. SIMON: I might cite one finding of rather general interest. The evidence was rather persuasive that we should separate within the accounting department the rou-

tine and repetitive record-keeping and reporting procedures from the task of maintaining communication with and performing special studies and other services for the operating men. Such a division would make possible a considerable centralization of record-keeping and pooling of personnel. At the same time, it would retain the real advantages of a decentralized analytical operation with competent men right down in the factory and at the various executive levels where they could deal with and be of maximum service to the operating people. This conclusion has broad applicability to a whole series of related organizational problems. How do you get long-range planning and analysis accomplished in the midst of the day-to-day pressures of management and supervision? The answer we reached—at least for this kind of accounting situation—is that you have to have a considerable measure of separation between heavy supervisory responsibility, and responsibility for long-range thinking.

Rather than describe other conclusions of this particular study, however—since it has been introduced here only to illustrate what we mean by research and the research approach to administrative problems—I wonder if we shouldn't look at some other examples of research in the area of administration. Dean Teele, you people at Harvard have been looking at some of these problems for a good many years. Would you care to comment on what you have found?

Some Conclusions Drawn From the Harvard Experience

MR. TEELE: We have tried to make a fairly sharp distinction between research and what I might call speculative thinking. On the research side, we have a number of policies. One of the most important is to devote one-third of our faculty time to research. In that program, I think you can begin to distinguish some threads that perhaps—I say “perhaps,” but I am really hopeful—will be more clearly distinguished when we look back at what we have done from the vantage point of twenty years from now. We believe that the first and most important test of a research proposal is that some member of the faculty wants very much to do the particular project. And since there are many differences among people, including members of college faculties, we’re certainly going to have a category of research activities that don’t follow any particular pattern. But I can see two other categories emerging. The first is in the area of administration which you have indicated as of primary interest to you in connection with this discussion. The second is a re-examination of economic theory, particularly about business firms, with the objective of finding out whether the blocks ought to be assembled a little differently. At the very end, these two categories come together, of course, but I think they are distinguishable at the outset.

You commented, Professor Simon, on the generalization drawn from the Hawthorne research and its confirmation in your own study. It has been our feeling that this kind of repetitive study is essential. We need not one or two or three, but ten or fifteen, related projects before we can begin to to draw firm conclusions. At Harvard, for example, we have

undertaken a series of related studies, some published and others about to be published, all in this area of what I would call administration, with emphasis on particular related but distinct problem areas. The most recent one deals with the problems encountered in adjusting people to technological change. What is the impact of a new product on all the people involved: industrial engineers, development engineers, production staff, workers? Although this is a study of a single situation, some generalizations are attempted. It would, it seems to me, be altogether desirable to extend this study of organizational response to change. Let's hope that fifteen more observations of this kind of problem will be made. Then, perhaps, we can hope to do what the physical scientists do: some person with imagination looking at the published studies, their evidence, their observations, will be able to say: "I think they fit together this way." Then others will begin to test his generalizations to see whether they do hold up in actual experience. *Then*, it seems to me, we shall have moved a significant distance toward the thoroughly documented type of conclusion that we associate with research in the physical sciences.

We still fall far short of realizing the application of scientific techniques to research problems in administration. But I have faith that it can be done.

MR. SMITH: I think an important distinction should be made between what we might think of as the method of the medical clinic and the method of the research laboratory. At one end of the research spectrum is the case study, or the multiple case study, based on specific situations, where the specific situations should never be divorced from the conclusions because they provide the only frame of reference for looking at the problem. At the other end of the spectrum is the really scientific piece of research, as the

physical scientists know it, where you examine a related body of facts with such accuracy that you can make generalizations that have validity apart from the specific situations on which they are based. The case that Herb Simon described, and the cases that have been examined most successfully in psychological research, pick out some activity that is common to a large number of industries and companies and is highly standardized so that you get a real degree of precision in comparing one situation with another, and then look at the variations in response. With that type of approach you can move more directly to the scientific study without this long preliminary period of the case study, the clinical situation as we might call it.

MR. BROOKS: As you look at top management problems, how many of them are of this standardized variety?

MR. SMITH: I'd like to hear more on that point from some of the business executives present. My own experience suggests that the appearance of great variety that is so marked as one first begins to look at business organizations hides the fact that they have a multitude of common problems that grow out of common functions.

Research Activities in Business Organizations

MR. COLLYER: I am inclined to agree with that. Before the discussion moves too far into problems of research from the universities' point of view, however, I want to report that there is a great deal more application and use of research in administration now practiced by business than was indicated in the memorandum you sent us, Dean Bach. Most companies with which I am familiar are very active in and encourage the kind of study Professor Simon described. My own company carries on what might be called business research in a number of fields. It seems to me that one thing

we need is a much wider dissemination of what has been learned by both industry and the universities.

MR. BACH: Well, the universities have printing presses at their disposal and have been known to use them. But I confess we know much less about what business itself is doing. May I ask you a question about it? Do you believe that your findings at Goodrich are of general applicability outside your own organization?

MR. COLLYER: I am not sure, but let me list a number of studies carried on in our organization. Some of them are just case studies, some might qualify under your definition of research. Like most companies, we make continual forecasts of general business conditions and trends. We make both long- and short-range studies of trends in other industries in which we are interested. We study single commodities, such as textiles and rubber. We study projected needs for and sources of capital. We study the potential economies deriving from proposed investments, market trends, sales potentials, competitors' policies and their implications, selling effectiveness of various components of our sales program. We survey the effectiveness of our magazine and television advertising. We use a wide range of psychological tests in the proper selection and placement of certain categories of personnel, and we study their results. We study organization structure within our own and other companies. We carry on a number of sales testing operations. And I don't think we are very different from many other companies which also have a wide range of research activities of this sort.

MR. KOHLER: I had the impression, Dean Bach, that your memorandum related to research in administrative practices, the making of decisions, that sort of thing. What Mr. Collyer is talking about may or may not have been in-

ferred. If the narrower interpretation is made of what the memorandum said, I, too, have the impression that much has been done in individual companies that has not found its way into print, either in magazine articles or in books. Perhaps a fertile field for someone to survey would be the extent to which, within certain companies, research has been carried forward that would formalize some of the ideas presented in your memorandum.

MR. KRAMER: I would like to add a bit to my earlier comments. My experience in attempting to find out what has been going on in the field of business for the past twenty-five years has led me to this conclusion. As I suggested earlier, there are certain areas of business operations that are pretty well known and pretty widely understood. If you want to find out something about production, about manufacturing processes, it's not very difficult to get what you're digging for. You can find out what the latest developments are in forging, in casting, in high alloy metals, things like that. It's about the same in the field of finance. You can find out a lot about the different types of financing that are available to a corporation, or relationships of banking institutions with manufacturing companies. It's about the same in marketing. Business law is pretty well explored. And in the last twenty years we have built up a wealth of information about the field of labor.

But it seems to me that there is still one area of business that has been little explored, and that's the field of business administration. I know that this has been our own experience, when we try to find out something that might be used in an article that we could label a "management" article, as contrasted with a "labor" article or a "finance" article. It is much more difficult to get that kind of information through interviews with business people. It is more

difficult to get even the basic information required to train yourself as an interviewer so that you can go out and ask intelligent questions. I notice that the literature on the subject of administration is still relatively meager. And that's why I think it's so important for research to be pushed into this area.

MR. SWENSRUD: Let me go back for a moment. I think it should be emphasized that most of our organizations do the kind of research that Mr. Collyer described. You might call it economic research. I don't think there are serious problems in this area. But when we come to the question of how our organizations function—there I think we have to face the fact that the kind of research we do within companies is generally devoted to solving spot problems against short deadlines. I think what Dean Bach has in mind is the fact that not enough research effort is being devoted, not to solving problems, but to extending the range of our knowledge. There's some of that sort of research going on, of course, both within individual companies and under the sponsorship of such organizations as the American Management Association. But most of it, I suspect, represents the contributions of individuals who start out working on some specific problem within their own company and then think they have discovered something worth communicating to others.

Mr. Collyer did mention one research activity that especially interested me. He said Goodrich did research in organization. I've noticed a few companies have what they call departments of organization. I assume they comprise a few individuals who are devoting their time to the study of their own organization and how it works and gathering information and ideas as to what other companies are doing and keeping up generally with what is being said and writ-

ten on this subject. Of course, in a sense that's the job of the president of any company. But he's usually trying to solve his own concrete problems. And he suffers the usual handicap of not being able to spare the time either to participate in systematic efforts to gain new knowledge about basic organizational behavior problems, or even to study the results of research that is going on elsewhere.

Researching Administrative Practices

MR. PRICE: I find compelling reasons for believing that administrative practice cannot be researched on a laboratory basis. It seems to me, rather, that such research must be directed toward the analysis of the effectiveness of different types of administrative practices in actual operating situations. From such studies of administration in operation, it should be possible to develop certain principles applicable to policies, procedures, and techniques that appear to be the most effective in specific circumstances.

I don't suppose anyone would deny the need for such research. Certainly the development of principles with respect to administrative practices has been sketchy and superficial up to this time. I would not hesitate to make this judgment even in the field of organization which has probably been studied more than any other aspect of administration. If you read what has been written on the subject and talk to administrators you find great divergence of opinion. The rudimentary nature of the conclusions reached may well be due, at least in part, to the fact that we have yet to discover practical and effective means for researching administration. With all apologies to Harvard, the case study method has not developed—was not intended to develop—universal principles with respect to administrative practices, although it has been highly effective as a technique for training future

business executives in the analysis of business problems.

The professional and business associations have not probed the field systematically and comprehensively. They're in business, yes, but like everyone else they have to have a product that can be sold. Their objectives, therefore, must be limited to providing guidance on specific issues of current urgency. Individuals who have attempted research on a limited scale have been handicapped by lack of help in conducting the type of project that cannot be financed by the sale of its results. They have also been handicapped by difficulties in getting access to organizations on the kind of basis that is required for valid research. The best approaches by individuals have consisted of the examination of the practices of an individual company or a very small group of companies, and these projects have suffered from being too specialized and unrepresentative.

It seems to me, in the light of this experience, that the schools of business and industrial administration are the proper agencies to conduct the research. They have the funds, the facilities, the analytical minds. And such research is consistent with their objectives—indeed, it's essential to the accomplishment of their objectives.

MR. KRAMER: How would you suggest this type of research might be conducted?

MR. PRICE: I'm really not prepared to be pinned down on this point. But I have a few suggestions as to approach. I think you have to aim at isolating the ingredients of success and failure in the principal phases of administration, incorporating the subjects mentioned in Dean Bach's memorandum distributed prior to this session. You could, for example, undertake to review the administrative practices of a large number of representative companies—representative as to size and type of business. An adequate sample

would, of course, be an important factor in the success of such a project because a common cause of failure in most of the work that I have seen has been limited coverage and inadequate representation. The work should be conducted by analysts familiar with the field of administration. The accumulated knowledge of experienced men who have retired from administrative positions in business would be valuable. As a minimum, the research group should be balanced in its composition, with ample representation of men with operating experience in the various fields of administration. It might be useful to place the project under the policy guidance of a board staffed by recognized, outstanding practitioners of the art of management. I am thinking of men of the caliber of Chester Barnard and Arthur W. Page.

The end of the research effort would be a report that attempted to isolate factors leading to success in each of the principal areas of administration. The conclusions on many points would, of course, vary according to the size and type of operation. Further, I'm sure that the research would not identify a single "right way" in any area. As a matter of fact, I suspect that one of its greatest contributions might be to destroy the mythical validity of some of the axioms that have become rooted in our thinking about administration. The fact might be uncovered—a fact too little recognized by administrative experts generally—that the number of ways of skinning a cat successfully are many and varied, depending upon the man who does the skinning, and perhaps also on the particular cat being skinned.

CONTRASTING RESEARCH PROBLEMS IN THE PHYSICAL AND SOCIAL SCIENCES

MR. BACH: Your suggestions, Mr. Price, highlight some of the very real observational problems we have to lick

if the thing we are researching is a human organization. Mr. Guetzkow here has been dealing with some of these same problems, I know, in work he has done on the success and failure of administrative conferences. I wonder if he would care to comment—particularly on contrasts in method with the natural sciences, where some of these observational difficulties can be met by laboratory controlled experiments.

MR. GUETZKOW: The problem we tackled was the attempt to isolate the factors that contribute to the success or failure of small group meetings, measuring success by the extent to which the results of a meeting are consistent with its objectives. We carried on the research by studying about one hundred decision-making staff conferences of groups of people in both industry and government. The researchers sat in the meetings and observed what took place; we also did some interviewing with participants before and after the meetings.

We got into problems of method right from the start, and I think they throw a good deal of light on some very specific difficulties that you encounter in conducting research in the social science field. The first problem of this sort that we faced was how to evaluate the outcome of a group meeting—were the results good, bad, or indifferent? If the objective of a meeting was to reach a decision on some issue, then the criterion for appraising the result logically should be the quality of the decision reached. But the men who were most competent to judge the quality of the decision that resulted from a meeting were usually participants in the meeting. Now, when the judge of results is also a participant in the process being evaluated, you face a problem in methodology that is altogether different from what you are up against when you have a measure of outcome or productiv-

ity that is completely independent of the operation or process being evaluated.

When we tried to use more objective kinds of criteria—such as judging the success of a conference by the number of decisions reached per unit of time—we faced problems of over-simplification. Do you get a better appraisal of the results of a conference if you aim at qualitative assessments of decisions, using the opinions of participants, or quantitative assessments that invite the question of whether you are hopelessly degenerating the criteria into meaningless numbers? We have some ideas about this issue, but I cite it as illustrating one type of problem commonly encountered in the social science field, namely, how do you establish criteria that are objective in the same sense as the chemist's criteria for measuring what happens in a test tube or the engineer's criteria for measuring what happens to a piece of metal under stress?

A second problem was connected with identifying the factors that influenced the outcome of a conference. In the natural or physical sciences, the researcher is frequently able to work with as few as five or six variable factors that influence a single process or operation. In our observations of conferences, however, we identified some 120 different variable factors that appeared to influence success or failure. In this type of situation, you simply cannot work with the precision of the chemist, the physicist, or the engineer. You have so many more variables under observation. And you don't have the facility of the physical science laboratory for isolating the more important individual variables. The result is that no single variable can possibly account for more than, say, ten percent of the outcome—much less, in most cases—and that throws you right back to the problem of the precision of your final assessment.

MR. W HYTE: With all these troubles, did you reach any conclusions about what influenced the effectiveness of conference procedures?

MR. GUETZKOW: Well, only partial ones, I'm afraid. Using more than one hundred variables we found we could account for perhaps 40 percent of the outcome in terms of the quality and number of decisions made, perhaps 60 percent in terms of a measure based on the satisfaction experienced by participants.

That brings to mind one more problem of method: that is the complication introduced by the interaction of factors. This, of course, you get in the physical sciences, as well. We found some very interesting interaction effects. For example, we found a close relationship between the satisfactoriness of a meeting, as judged by the participants, and the extent to which the leader controlled the *procedure* of the meeting, without tampering with the content. But we found that you could wash this factor out if you got a group of participants that was highly united and attracted to the purpose of the meeting. Then it didn't seem to matter what the leader did or how he behaved. The members were still satisfied or dissatisfied without reference to the leader's behavior.

MR. HAZARD: As I understand what you have said, you are suggesting that it is possible, in social science research, in management research, to do a job that is reasonably precise, so that you can put your finger rather exactly on what is going on and know rather exactly what results you are getting? And that this is true even in this especially elusive area of human behavior?

MR. GUETZKOW: That's right. Of course, we are still looking for techniques that will make it possible to account for the other parts of the outcome—but we have a

long way to go. I don't mean by that that fundamental research should try to explain quantitatively how all of these interacting factors will apply in a given concrete situation. Depending on the circumstances, the individual factors at work are going to enter with very different weights in different situations. Those weights have to be found in the specific situation—that is the task of application, not of fundamental research. The universities are equipped to research on the fundamental problem, but that still leaves a big job for the social engineer—to apply the fundamental knowledge to immediately practical issues.

MR. McDANIEL: If you make that distinction, then you have gone a long way toward determining the nature of the work that you should do as a member of this faculty.

MR. GUETZKOW: Our job as social scientists is to understand the variables and the mechanisms involved, and to test them and explore them in controlled situations.

MR. McDANIEL: Your proper sphere of activity, as you see it, is to create the model. The application of that model to specific problems then becomes the work of those men who face short-range, practical problems.

MR. KOHLER: That's a valuable summary you've made. There are a couple of related points that have occurred to me in connection with the analogy that might be drawn between the work of the social scientist and the physical scientist. There is a parallelism both in experimentation and in invention. In dealing with these two processes the head of a research group in the physical sciences is given almost a free hand. It seems to me that in the social sciences a great deal more attention needs to be paid to invention. As in controls, for example, within business and over business—why, we haven't even begun to explore the possibilities of

invention. Of course, with invention goes experimentation in order that choice can be made between different schemes or different plans.

But it seems to me that the analogy carries even further than you indicated. The basic processes followed in the physical sciences can be brought over to a considerable extent into the social sciences. The exact methods, of course, may differ, as you indicated. In the physical field you may have six variables within which you're supposed to come to a conclusion. In the social sciences you may have 120 variables and that may not exhaust the entire list. The next day there may be 120 more.

One problem of that type that I've observed is the variation arising from the introduction of a new personality into an existing situation which is about to be resolved. Maybe by sheer accident, this new personality brings in with him new factors which completely upset the diagnoses or tentative conclusions that had been arrived at, thus necessitating a completely new investigation and new conclusions.

The point I'm making is that the analogy goes quite far. The methods will necessarily differ. But I think the engineers and the mathematicians have a great deal to teach the social scientists. They have gone through a series of processes that the social scientist himself must go through. The social scientist stands to profit by the mistakes that have been made, by the methods that have been developed by the physical scientist in the course of his extensive experimentation. I don't think there is anything the physical scientist does that shouldn't be taken advantage of by the social scientist to the greatest possible extent. I think the social scientist must at all times be aware of the methods and the scheme of thinking that go on in the minds of the physical scientists before he can make the most of his own potentialities.

HORIZONS AND PROBLEMS

MR. HAZARD: I am still far from convinced that you can let your thinking be guided entirely by the delightful prospects of adapting the methods of research in the physical sciences to management problems. You can get a chemical in a test tube or an atom to stand still—I may be offending my scientific friends by this figure of speech, but it will be useful to make this point—and you can study it and find out how it behaves and what it does, and then you can draw certain conclusions and finally apply your conclusions. All this is going on in a controlled environment. But to apply the same technique to a business problem, where the environment in different organizations and in different circumstances is not always uniform, so that you have to come up with a conclusion that may be reliable only in two-thirds of the cases—well, this may be extremely misleading, it may even be extremely dangerous. We all know the story of the surgeon who reported that the operation was a success but the patient was dead. I don't think you can afford to go all-out along the line suggested by the physical sciences. You must use a lot of discretion.

SUMMARY COMMENTS ON FUNDAMENTAL AND APPLIED RESEARCH

MR. SMITH: Let me go back a moment. It seems to me that we have been raising a series of important points that bear on the issues connected with defining fundamental research; understanding its relationship to applied or practical research; exploring the place of a university, its people, its facilities and skills, in carrying on research; and similar matters. It may be useful to try to put these issues into a series of steps that represent an appropriate action sequence. To solve problems you have to have facts. Not just any odd assortment of facts, but the right facts, the pertinent facts.

In order to get the right facts you have to ask the right questions, or your facts will mean little and will be no more than a collection of miscellaneous data, one of the most pernicious forms of time-wasting in research. So you start out with some such question as the one that started Herb Simon off on his research project: the extent to which decentralization is desirable in performing the accounting function in a large organization. As you go along, you collect some data, and in the process of doing that you deal with problems associated with the character of the data and ways of collecting data.

At that point the road quite often forks. The next step can be guided along two different paths. One is toward immediately useful, practical short-term results. Curving the research path in that direction, it seems to me, means that what you are doing is applied research and is probably not a proper university function. In most instances, this is something the individual company can do much better by itself, or it can hire a practical-minded group, such as a management consultant firm, to do the job for it.

The other curve is toward generalization, toward broad applications. It involves techniques that the university staff can use with rather greater assurance than the individual company, and it usually calls for ways of thinking and analyzing that are more likely to be found among university staffs than in business organizations. This path leads toward fundamental research.

When, for example, one starts with the location of the accounting function, as Herb Simon did, and rather early gets involved, not in specific answers to location questions in specific companies, but in the much broader problem of communication and interaction between a group with one set of concepts and mores and a group with another set of con-

cepts and mores, and then perhaps moves from these generalized issues to the still broader problems associated with the ways in which decisions are made and carried out in complex human organizations—then one is deep in fundamental research. And if any one is equipped to do this kind of research, the universities are. I feel profoundly that it is this path that we must follow in planning our research efforts. We must concentrate on the things we are equipped to do better than business, and leave to business the problems they are better equipped to work at.

MR. MCDANIEL: May I comment on two things? First: about research. I feel that there is confusion in our use of the term. The word is being used in different ways. We have talked as though the physical sciences have developed a research technique that operates perfectly. Probably the greatest revolutionary changes in techniques in the last ten years have occurred in the natural sciences. I am convinced that many techniques formerly used are no longer applicable. I can remember the time in school, and so can all of you, when in chemistry you were presented with what was called the periodic table; you were told that that periodic table listed the elements; then you were told what an element was. It was an irreducible thing, and there it was.

Physical chemists have thrown that away. There's a completely new orientation. Many things that were formerly accepted as ultimate facts are no longer so accepted. There's a whole world of new things. I think we must be very careful not to assume that the physical and natural sciences have solved all their problems and that their techniques have been perfected. What is important, it seems to me, is that we understand and understand how to use the methodology that has been developed in the physical and natural sciences. This is a matter of collecting data for a specific purpose.

You make observations; you reach tentative conclusions; you test those tentative conclusions; and when your observations are sufficiently numerous and your sample is acceptable, then you dare to move on to the point of expressing your conclusions in the form of a law. What is important to us is the methodology of problem solving.

I have a second point. This is a matter that I find very disturbing. I have difficulty in expressing it and if I sound a little confused, I hope you will bear with me. As I listen to this discussion I feel a sense of compulsion—an insistence that you must do research, that this particular institution must do research. Not only does this institution have to do it, but every single man on its faculty must do research. Let me say frankly that I don't know whether all the members of the faculty of this or any other institution are capable of doing research. They may be. But should they do so because they feel compelled to do so?

In any event, it seems to me that before you can talk constructively to business men about problem areas into which research should be directed, the individual faculty member should decide for himself what kind of research he wants to do and is capable of doing. Not until he has resolved this issue should he approach business asking it to guide him in his research activities. The key questions are: What competence do you have? What facilities do you have? What can you adjust yourselves to efficiently? When you have answered these questions, then, I think, is the time to approach business men; then you may say: "These are the things we can do competently. We should like to do them through your organizations or in consultation with you."

MR. BACH: I could give you a long answer to that observation, at the risk of cutting off what we hope to gain from other parts of this discussion. Let me give you a one

minute answer instead. We have done a good deal of the kind of self-analysis that you suggest. I can assure you that we at Carnegie know what we want to do, in the way of research, with one big chunk of our talent and time and money. But one of the points that we have been concerned about—which may have led you to the conclusion that you have just indicated—was our feeling that we ought to pause for a moment in our work, and sit back, and re-examine this whole research business to find out whether we are moving in the right direction. I think this feeling exists not only at this school, but in many other schools that have research facilities and ambitions.

Perhaps this suggests that we are ready to turn to one of the other important topics on which we think you business men can be of basic assistance in helping us with the re-examination of our effort. I wonder if we shouldn't talk over some of the important areas toward which research should be directed. We are deeply interested in hearing from you men in business about the problems that you think require researching, and that may be approached with some of the research techniques that we have been talking about.

Areas for Research

RETIREMENT

MR. ROBINS: I happen to be chairman of a sub-committee of a national association that has been given a comprehensive assignment of taking a fresh look at problems associated with old age: particularly the economics of financing old age—the national issue and the industry and single-company issue. We have some very able men on the sub-committee who are experts in the field. Among other things, we've had extended discussion of compulsory vs. non-compulsory retirement at age sixty-five. The experts tell us they haven't found any way to decide scientifically when a man should be retired or down-graded because of advancing years. That's a mystery to me. I don't happen to be a personnel expert, but if a personnel organization can scientifically select men and scientifically up-grade them over a period of forty years, it would seem to me a smaller problem to face the issue of how to down-grade them or retire them. Nevertheless, first-class people claim it's a major problem. It seems to me that here is an area where a school of industrial administration could do an outstandingly important job of basic research. And this is one more example of a problem that stretches far beyond the horizon of the single company and the specific situation.

MR. SWENSRUD: This retirement problem is certainly very important. I recall all sorts of arguments about it, but the best one I've encountered is that if you don't retire every one at the same age, you have a terrific morale problem based on alleged discrimination. If exceptions are made, how can you convince the man you want to retire on the regular schedule that your judgment is really as objec-

tive and scientific as you claim it is? I think it would be extremely useful to have a compilation of what has been tried and what the results have been. This is close, Dean Bach, to the question raised in your memorandum: why does something work well in one situation and not so well in another?

MR. HAZARD: You have the same problem of selection when you up-grade people.

MR. SWENSRUD: Of course you do, but there you have to make a choice and every one knows it. You don't have the same compulsion in retirement.

MR. HAZARD: The real difficulty would appear to be that chronological age is no indicator of the efficiency of human organisms. You can have an old man of thirty-five, or a young men of seventy. Furthermore, if you retire a young man of sixty-five, force him out of his pattern of life, he often gets sick and becomes a health problem. This raises the issue whether the economy can afford to keep people who could be productive not only in idleness, but also in ill health.

MR. ROBINS: It's an important problem and one that is growing rapidly, especially with the growth of pension plans. Many managers don't see its broad implications. We're just beginning to work our way out of discrimination in so many other areas: on the basis of race, color, religion, country of origin. And now we find ourselves on the threshold of developing another huge basis for discrimination: against people who have reached a certain age. And for no other reason than their age. And on a whopping big scale. It's controversial in the extreme and it's very expensive. I think this whole area is wide open as a rich field for the kind of research you people are undoubtedly equipped to do.

MR. KRAMER: Just to satisfy my curiosity—how did we decide that sixty-five should be the automatic cut-off point?

MR. HAZARD: I can tell you the answer that was given to me. This retirement concept arose in Germany as part of the broad social movement of the last half of the nineteenth century. It was opposed by the property owners. They didn't like the idea. So they fixed on sixty-five because very few people lived to be that old then. The cost would be negligible. Since that time, of course, medical science has increased our life expectancy, and here we still have the sixty-five figure with a real bite now.

MR. KRAMER: Does that suggest that it would be appropriate to move the retirement standard back to seventy?

MR. COLLYER: I think research will have to tell us that. But this suggests another important point. We have compulsory retirement at age sixty-five in our company today. And our managerial group is persuaded that in nine cases out of ten when we retire a man and move someone up to replace him it causes heartaches, but we strengthen the organization.

MR. SWENSRUD: I'm glad you made that point. I think that in any research on this problem a distinction should be made between managerial personnel and production workers. In the managerial area there are important problems connected with maintaining fluidity in an organization and avoiding the kind of hold-back and damming-up of younger men that occurs when you keep older people in their jobs too long.

MR. TEELE: I think this is a crucial area for research. A couple of years ago, in association with a number of life insurance companies, we held a three-day conference on

"what, twenty years hence, the life insurance people will wish they had thought about now." One of the important issues that emerged from that discussion was this one of retirement. Where will we be if we persist in reducing the retirement age at the same time that medical developments are prolonging the span of human life? This is an economic and social problem of tremendous importance. Incidentally, Sid, we made the same division between factory workers and executive personnel that you suggest. We undertook some research on the subject and the results will be published shortly. I think that the clearest conclusion from our work is that an immense amount of work remains to be done. We were talking earlier about the difficulty of generalizing. I have this especially in mind, because our studies represent only a first attack at a few situations and we need much more extensive exploration before we can approach safe conclusions on which reliance can be placed.

MR. SWENSRUD: You can't divorce the problem from labor organization and labor relations, either. We have not had a definite retirement policy in our company, although we have been working toward one. And in doing so we have found that we are forced to consider a number of things that didn't appear on first examination of what was involved. We bumped into the labor issue in one of our plants that does not have an organized bargaining unit. Quite a few of the men were getting along in years and the manager thought some policy ought to be worked out. He came along the other day and said he wanted to put a notice on the bulletin board announcing a definite policy. He had tried to work the problem out on an individual basis but found that he was getting too many repercussions. The union leaders were seizing upon this issue as a source of complaint.

MR. ROBINS: There's another aspect that takes on prominence when you consider the problem from the viewpoint of human relations. Industry generally has accepted the proposition that it should assume the responsibility for hiring and using handicapped individuals—lacking legs, arms, and eyes. We've made a lot of progress in this direction in recent years. Finding ways to employ the handicapped is becoming part of the social mores, you might say, of the country. And here we are talking about another form of handicapped people, those handicapped by old age, and the question is: shall we try to hire handicapped young people while forcing the retirement of handicapped old people?

PERPETUITY OF ORGANIZATIONS

MR. BROOKS: May I say that there is another compelling reason for looking into this problem of retirement: that is the urgency of opening paths of opportunity for young men. You know, they say one of the characteristics of the business man who goes into the academic world is that he tends to become even more theoretical than the academicians are alleged to be. Let me step back to the business side of my life for a moment. The high command of industry, management as we call it, certainly has a responsibility for getting results. But the high command of any organization—whether it's church, army, or business—has another urgent responsibility and that is for the perpetuation of the institution. Much of the research that Sears carries on is concerned with this subject of continuity of effectiveness. We certainly know any number of cases where a company has gone into decline. I think we could, for example, point to the railroad industry as a whole industry that hasn't maintained the virility and drive and power for

growth that marked it in earlier periods of its history. Nor has the result been the same for all companies in that industry. The meat packing industry—or some companies in it—offers another example. I have a notion that a lot needs to be found out about the elements of decay that creep into an organization and lead to deterioration. There isn't a business man in the world who looks with favor on the word "bureaucracy," yet the deadly influence of bureaucracy slips into many organizations unseen and unheard. It's a most insidious development and, in my opinion, it is one of the principal reasons for decay and decline in organizations. And I'm not necessarily talking exclusively about business organizations.

This suggests to me that it may be important to point the research spotlight at some of the older human organizations that have a history available for study. The church, the army, the educational institutions have to get results, too, just like business. Now the average business man may drag out that old cliché about their not having to meet a payroll. But they do have to get results, different results. We have to find out what vitamins are eaten by the organizations that seem to have learned some of the secrets of perpetuation. There is a tremendous field for research in this whole subject of perpetuity in industry. I hope I'm not being too theoretical for my ex-colleagues in business?

MR. ROBINS: On the contrary. But I would like to know how you would do this in the case of individual companies. What you want to do is look at a dead company and try to find out what killed it. You would probably also want to look at some companies that seemed to stand almost motionless for a long time and suddenly began to move ahead. It's the problem in reverse. But I should think that would be considerably easier than walking into a company

that seems to be in the process of dying and say: "We understand that you're dying and we want to see how near dead you are and what seems to be the cause."

MR. BACH: Mr. Whyte, I know that you people at *Fortune* have been concerned about what values there are in organizations and what forces keep them growing, or, in reverse, create a setting for deterioration and decay.

MR. WHYTE: There are several observations I'd like to make, Dean Bach. I think they relate, directly or tangentially, to both your question and some of the preceding discussion about perpetuity which I've found extremely interesting.

In the course of pursuing the material for a story, we found that the college class of 1949, and the class before it and the class after, were the most group-minded collection of young people that we have ever known. They are tremendously interested in security—security as against opportunity, let's say—and not only financial security but emotional security, as well. When you translate this into kinds of jobs, you find that there has never been such an urge on the part of a whole college generation to get into staff as contrasted with line positions. They wish to be technicians of society more than leaders.

Above all they are group operators; there probably has never been a generation so dedicated to the task of *getting along*. We encountered what appeared to be other aspects of this same group-minded phenomenon in a couple of apparently unrelated stories that we were investigating. I'm troubled about the broad implications of this, without quite understanding what it may mean or what the answers may be.

We know that all our organizations have a pronounced tendency to bureaucratize themselves, including journalism,

I might add. If you put that together with this observed desire to get into staff positions and the apparent lack of interest in opportunities accompanied by risk as against security—you may begin to wonder what dangers may be implicit in the collective situation. I wonder if all this is related to the idea that seems to be becoming more popular, that one of a company's main duties is to provide social satisfaction for the members of its organization. If you consider this from the viewpoint of the college generation to which I referred, it seems to be precisely what they want. Is this a good thing? Should a company reach out that far to provide satisfactions of a social or group sort? May there not be some dangerous kickbacks residual in this movement? Is all this related to Dean Brooks' concern about perpetuity and dying industries and individual firms?

Again, we got the feeling that this college generation is more concerned with becoming effective group "operators," highly skilled socially, knowing how to get along well with people. There is greatly increased interest in this same problem among persons in industrial relations work. There is really a fantastic amount of work, of various degrees of usefulness, in progress on how people get along in groups. Are we not in danger of overlooking a critical new problem—how do we provide a favorable environment and proper incentives for the independent spirit? How do we nurture that type of individual? He may not fit at all well into groups. He may be almost destructive in his interpersonal relationships. But he may be that rare individual who has the genius and the drive that can spark a sleeping organization, who can really make the difference between life and death for a firm.

We made a study of one company, a rather small concern, that has gone whole-hog for this human relations ap-

proach. They have a small research department and we were talking with the people who run it. They're very upset. They feel very strongly that the company should provide full social satisfaction for its young members. They all work so terribly hard getting along with everybody. They hold conferences all the time. But the president of that company told us: "We're worried about the head of our research department. He just doesn't seem to be able to produce new ideas, he's not nearly the equal of this young fellow we have in our office." But there was conflict. If they fired the head of the research department, or moved him upstairs, and brought in this young man to replace him, they would be upsetting all the internal relationships of the organization. And on the other hand, if they didn't promote the younger man and give him an opportunity, his growing antagonisms toward the research chief and his own frustration would compel them to let him go. They finally let him go. They were losing, and they knew they were losing, a dynamic individual who could have meant a great deal for the future of their company.

There's a real research problem, I feel, involved in finding out how, in the midst of this defined trend toward bureaucracy that may be inevitable in large organizations, you find the independent spirit, and keep him, and encourage him.

FINDING, TRAINING, AND MOTIVATING EXECUTIVE TALENT

MR. COLLYER: Dean Bach, this is a subject of great concern to me. I would like to state what, in my opinion, is one of the most urgent needs of our company. I don't know whether it offers a field for research, but it may. That's for you to judge. First, how can potential members of management be more readily discovered in an organiza-

tion? Second, what on-the-job and off-the-job training should be offered for their development after they have been discovered? And third, what are the best incentives and rewards under today's conditions for maximum accomplishment by members of management?

MR. TEELE: May I say amen, with this specific addition? For several years now I've been watching the executives of many of your companies in our Advanced Management Program. I sat, for example, yesterday in a class with a group of them. These men have an average age of 44, average business experience therefore of 22-23 years, average salary of around \$20,000. So you people have shown some confidence in them already. And I wonder if you realize as strongly as an outside observer does how thoroughly specialized and narrow these men are. It is incredible. We haven't done the things with those men during these twenty years that we should have been doing. It isn't a matter of profound knowledge of complex problems in business and government. It's that they can't read a balance sheet. An operating statement is a mystery to them. Now these are good men, but in one limited field. And these are the men that American business is going to depend on.

MR. HAZARD: I think this is of outstanding importance. I've certainly noticed it right in my own field, in the law. The specialization that has taken place is shocking. And I don't know how we can avoid it. But it seems to me that it points up all the more the necessity for research in what top management really is. We're all obsessed by expertness. I've heard business men say that if they want to do such a simple thing as make a statement to the press, by the time it gets through the public relations people, the tax lawyers, and so on, there isn't much left. An expert, they tell me, is one who makes no small errors as he proceeds to the grand fallacy. Nevertheless we must have experts. And therefore it

seems to me all the more necessary to do fundamental research to discover the hard core of the top managerial principle. It does sound vague. But what you're really looking for is what Harvey was looking for when he found the circulation of the blood. We've got to find out what is at the very heart of the business mechanism.

MR. KRAMER: I was talking recently to a man who had just become president of a company, and he described it as the most frightening experience he had ever known: suddenly to shift from vice-president in charge of manufacturing to president. A month after he was on the job the sales manager came in and laid on his desk a \$3,000,000 advertising budget and said, "Will you approve this?" And he replied, "I don't know yet." But he has to know.

MR. WHITE: Let me toss another one in. In the course of pursuing another story we talked with a group of business executives enrolled in an advanced training course. They were all between the ages of 35 and 40. We weren't interested in getting this particular bit of information, but it came out in the course of the interviews and we followed it a bit. We got into the question of ambition in a sort of a long bull session with them. We said, "Obviously, top management has tapped you for promotion, otherwise you wouldn't be here. Where would you like to be ten or fifteen years from now?"

The answers that came back just about floored us. Most of them said in the first place they didn't want to be in this particular school. They knew it was a bit of a promotion. But they simply didn't want to go much further in their companies. They came out with words like "merry-go-round," "treadmill," phrases like, "sure, you've got to keep up; you don't want to fall behind." To put it frankly, they just didn't sound very ambitious. One of the men there, a

rather unusual fellow, said to us, "It's funny. I don't know whether you fellows have gotten the same idea, but if this is a sample of business management, we're not very ambitious any more. Or if we are, it's in entirely different terms. I'm completely confused." It certainly confused us. There was very little talk of money. It suggests the question: what are the measures of ambition?

MR. McDANIEL: And who are the people competent to carry on that particular investigation? Psychiatrists? Psychologists? Who?

MR. BACH: Some of us have a strong suspicion that nobody is really competent. But several different specialists working together may produce a kind of group competency.

MR. PRICE: I'd like to follow up what you said with this thought. After these men finish an advanced management training course, the Westinghouse Company finds it very difficult to place them in good spots for further development, because of that long period of specialization. Give us another five years and you'll get younger men, men who have not been in the company so long. They will be broader. They will have moved around among departments. They will have a much more varied experience. And when they come back they will be able to do something about it.

MR. BACH: The research problem underlying this is, I suppose, how do you find these men before they've gone too far in one part of an organization? How do you put your finger on them as likely candidates for special training?

SOME LABOR-MANAGEMENT ISSUES

MR. COLLYER: All of us are worried about how to improve our labor relations. It seems to me that there are some basic questions that have to be answered before we

can begin to understand how to proceed. What incentives induce people to work? Are such incentives now available? How can people agree on adjustments of wages and employment conditions without strikes? How can we improve the bargaining process so that it becomes truly a system for reconciling and compromising differences? Can differences between employees and companies be settled equitably and amicably unless the bargaining power of unions and companies is about equal?

Then there's another, although related, field in this issue of productivity. The General Motors Corporation has moved into this area with their contract which gives effect to changes in the cost of living and an automatic productivity adjustment. Yet we all know that increases in productivity often have nothing whatever to do with labor's contribution. They're the result of new equipment, or of the kind of research that leads to the discovery that by opening a valve a little more or at a different time, or by adding a new ingredient or changing the material mix, we get substantial increases in output or improvement in quality. But what have such productivity gains got to do with labor and what influence should they have, can they be allowed to have, on the employment contract?

SOME ECONOMIC PROBLEMS

MR. SWENSRUD: I'd like to mention another area for research, this one in the field of what might be called economic or monetary problems. Mr. Collyer spoke of the escalator clause in the General Motors wage contract. I know that General Motors has a special view of the effect on the price level of such clauses providing for automatically increasing wages. They claim there is no effect. Many people seem to think that the widespread adoption of the policy expressed in that type of wage agreement may cause a

steady inflation in the economy. What I want to ask—and suggest as a possible area for university research—is whether, if this is true, it would still be a cheap price to pay for improved employee relations? This may seem like a rather theoretical question on first view, but I think it is really intensely practical. If business men were to conclude that, regardless of what they think, there is going to be a certain amount of steady inflation in the economy anyway, and if they sensed a general disposition to believe that a certain amount of inflation may generally be a good thing, or at least have a stimulative effect with some good aspects, they might develop for their own companies quite a different set of policies than if they reached opposite conclusions as to general price probabilities for the future. Contrariwise, they might conclude that the benefits of increasing productivity ought to appear in the shape of decreasing prices rather than increasing wages. Now I believe that somewhere along the line business leaders as well as professional economists in government agencies are going to have to deal with this problem. Is inflation a good or a bad thing?

As business men we can't afford to forget, in thinking about our decisions, that we have certain rather clearly defined limits within which we can make decisions. A lot of decisions are made for us by the environment within which we work and from which we cannot escape. I certainly think there has got to be some real sound thinking on questions of monetary policy as they affect decisions, as well as about the decisions themselves and how they are made and carried out.

ORGANIZATION STRUCTURE

MR. JACOBY: So far today our discussion seems to have given research in administrative organization a rather rough time of it. There has been some comment that it's not

fruitful, that it's not the business of the academics, that it's not fundamental. I'd like to call attention to the importance of work in that area and to the need for it. We've been praising the habit, in the physical and natural sciences, of asking why things should be as they are, of challenging the *status quo*, as it were. Very few of us have been ready to ask that question in the field of organization. There's a good reason for that inertia. It's not easy to wrench ourselves out of a pattern of thought. We have vested interests, perhaps, in existing types of administrative organization. But I think there may be substantial gains to be secured from not being afraid to take a hard, candid, critical look at some existing types of organization.

I understand, for example, that Sears Roebuck is now carrying on an interesting experiment in organization. They have been exploring the utility of the idea of very large delegations of responsibility way down the organization line, a flat type of organization with tremendously wide spans of control, as the organization theorists put it. This is really quite unorthodox in terms of common ways of thinking about organization structures. But they're trying to see what happens in such a set-up. Does it produce results? What kinds of results? I don't know that any conclusions have been reached as yet. But this is a good example of a company that is conscientiously asking itself the question: why? It seems to me that more companies ought to ask themselves that question with respect to their organization structure. And I am persuaded that schools of business can make a valuable research contribution by working with companies in trying to answer this question.

I am concerned that very few companies seem to have set up staffs to do research in organization structure. They have economic research staffs, market research staffs, all

kinds of technical research groups. But not in the area of organization structure. The growth of giant organizations seems to demand intensive re-thinking of the whole problem of whether present organizational patterns are appropriate, or should be remoulded. As an educator I have no answer ready-made for this question. But I am persuaded that there are important needs and great opportunities for research in this area.

MR. KOHLER: But don't you think that such research must be preceded by a great deal of data collection that hasn't been done by anyone? A survey was made under the sponsorship of Stanford some years ago; the findings and conclusions were published in a book called *Top Management Organization and Control*. Summarized in the book are the top organizational structures of some thirty-odd corporations, representing large companies in various fields. According to one of the men who conducted the research, the attempt was made to explore no more than three layers of authority, starting from the top. They did this to avoid becoming engrossed in detailed problems of organization and operation, and to confine themselves to the area where general management functioned. What they observed is reported in the book. To my way of thinking, what they describe is more important than the conclusions they draw from their observations. The conclusions, summarized in a separate chapter, are generally commonplace and uninteresting. The reason for this, I believe, is that they had too few cases on which to base their judgments. Which brings me back to the point that has already been made by others here, that at this stage we ought to be more concerned with making observations, collecting facts, if you like, than with drawing conclusions or generalizing, in the language of the researcher.

Problems in Carrying on Research

RESEARCHING THE DECISION-MAKING PROCESS

MR. ANSHEN: Much of what we have been saying up to this point has pertained to these related questions: What kinds of management problems are researchable? How can the research approach be brought to bear on these problems in ways that will be useful to administrators? What problems are likely to be encountered in undertaking such research? I wonder if it would be worth while to consider step by step just how this would look if the group assembled here were planning the undertaking of a specific research project.

One problem that has been of great interest to us at Carnegie is: How does a major policy decision get made in a business organization, and, after it has been made, how does it get put into execution? Both business and university people have thought about and chewed over many specialized aspects of this problem. I have in mind such questions as line-staff relationships, how to get the essential facts organized and analyzed and presented to those who make decisions, communication between organization layers, formal and informal organization structures, the translation of broad policy into specific operations, and a number of others. All of these issues come into focus in terms of the process of making and executing a policy decision. This is the organization in action, so to speak, with all its strengths and all its weaknesses, all its tested methods and all its unsolved headaches.

Would it be valuable and informative to you as managers to have this process studied in terms of the formation and execution of a specific policy decision in one concern?

Would it contribute something important to management's ability to understand how its job gets done and how it might get done better?

Suppose some of our faculty members worked out an arrangement with one of your organizations to try to study the decision-making and executing process in a company in terms of a single major policy decision selected as a good test case. How would your team and our team work together on the problem? What kinds of difficulties do you think might be encountered? What would be the best way of carrying on the research? What would be the best way of getting the cooperation of your people? Into what areas should the study be pushed to be of greatest value in throwing light on the whole decision process and exploring some of the weak points in the operation?

MR. BACH: Here we have a whole hat full of questions. Let me pick just one out to put to the group. Do you have the feeling that an outside group can work effectively within a business concern on this kind of a problem? Can we form joint teams that will work effectively together as research units? Just how much monkeying around of this sort will business tolerate? What will the researchers be permitted to observe? How far will they be permitted to probe? Under what circumstances will companies actually allow experiments with their administrative arrangements?

Dean Teele, you people at Harvard have a vast experience with this problem.

MR. TEELE: I think the answer depends on several variables. One, of course, is the kind of problem you are attacking. In one sense, you always have a team. Unless you get some individual or some group within a company sufficiently interested in a research project to the point that they do participate, very little happens. I take it therefore

that you are thinking of more formal associations, and on that I don't think it is essential unless the project requires a long stay in a single company. In the case of the famous Hawthorne research where there was a formal university-company team, the group was originally set up, as I recall it, for six months. It went on for several years in the same plant, and the by-products have been pursued for fifteen years. I don't suppose you could do that kind of thing without a formally organized team to carry it on.

I was commenting at lunch on a current experiment of ours that bears on the point under discussion. We have been trying to get observers so immersed in a business situation that they become almost a piece of furniture in the office. That takes a long time. We have set up in two plants on what we think of as a permanent listening post basis, with the objective of getting a group of observers who will gain acceptance rather faster than a single person. We are hopeful that with several people going in and out, each one of them will be accepted more quickly and will become neutralized in the environment.

This kind of thing obviously can't be done except on a partnership or team basis. You will have to build a degree of interest on the part of the business firm that results in their becoming, as it were, almost partners in the enterprise. On the other hand, the survey type of research that we were talking about earlier—where you go out to discover current practice in a large number of companies—so that you can conclude that what is being done in, say, eighty-seven out of a hundred companies is a reliable sample from which to generalize on common practice—here, there is no need for the team set-up.

MR. PRICE: I'd like to go back for a moment to another question Mr. Anshen raised: really his fundamental

proposition about researching the decision-making process. Do you think you are going to be able to find tangible materials to put together to throw light on that kind of problem?

MR. ANSHEN: I don't know. This is the kind of problem that I think we ought to explore together for a few minutes.

MR. ROBINS: I should think it would be a terribly difficult assignment. Suppose you were to tackle a research project aimed at finding out how a company came to make an important series of decisions, say the Anglo-Iranian Oil Company and the decisions on how they would deal with Mr. Mossadegh. I wonder if you could emerge with any real concept of how their decisions were actually made? There's a terrific amount of the subjective, the personal, the emotional, involved in many of these major decisions. Is there any way of getting at that?

MR. SWENSRUD: I should think it would be terribly difficult. I've tried it myself, in reading some materials related to anti-trust proceedings, to determine why this or that move was made.

MR. BROOKS: I am wondering if contemporary studies of how decisions are made at the very time they are in process of being made would be more revealing than studies of how decisions were made in the past.

MR. KOHLER: There's a related question in my mind which I have often considered and I suspect others sitting around the room have, too. That is the definition of a policy decision. That, to me, is a question of considerable importance. I regard such a decision as one that includes the carry-through, the action that is necessary to put the decision into effect. Then, after the action has been taken, there's the question of the so-called feed-back, the information that

comes back to the person responsible for carrying out the decision, which may result in a need to modify the decision. There's a whole of a difference between arriving at a top policy decision, and translating that decision into action. The definition of decision to be used for such a research undertaking, I suggest, should include these two additional elements: the carry-through, and the feed-back.

MR. SWENSRUD: I agree that there is an important area for research in this issue of decision-making. But I am far from clear about whether policies grow out of decisions or decisions out of policies. I am also far from clear as to how far the decision-making process can be made sort of "automatic," or at least less personal. With some decisions, you have to reach conclusions very quickly because there is a deadline hurrying you along. These, I am sure, are *made* decisions, and they may have to be personal within the grasp of the top executive. With others, you have more time, and it has been my experience that if you let the people who are best equipped to do so throw light on whatever problem is under review the right decision will often emerge more or less automatically out of the process. No matter how difficult a problem may be, if you keep working at it, it's surprising how often you can narrow it down to where you finally know what the right decision is. And I have a suspicion that a valuable research project could be undertaken to throw light on this decision-making job, particularly where the decision responsibility is not of the short-term, personal sort that we talked about.

QUANTITATIVE AND QUALITATIVE FACTORS

MR. JACOBY: I have a couple of thoughts on this problem of decision making that I'd like to pitch into the discussion. It seems to me that when we speak of decision

making we are concerned with at least two categories of problems. First, who makes the decisions? What are the participating groups in the organization and how do they participate? Second, on what premises are decisions made? With what values? Within what frames of reference? With what evidence? Can the quality of decision making in any organization be improved by a more effective allocation of responsibility for decision making?

Let me put some of these questions into the framework of a specific illustration. A couple of years ago the task fell to me to write a paper on the subject, *Factors Influencing Management's Choice of Forms of Financing*. My assignment was this: Suppose a business firm needs X dollars of new capital. (We won't for the moment debate the equally interesting question of why it needed X dollars and not some other figure.) My problem was to present a rational formulation of how the management should finance its need. In what form should the money be raised: stocks, bonds, short-term debt, term loans, and so on. Of course, an economist could sit down and study the problem and emerge with any number of factors that he thinks may or should be influential: the asset-liability structure of the firm, the amount of debt outstanding, the cost of financing through equity instruments or borrowed money, the degree of exposure to risk involved in assuming fixed charges against future income, changes in the distribution of power that may be involved in different types of financing, and so on. You can extend the list at your pleasure.

But in the end you come to the question of how all these factors can be appraised and assessed. It seems to me that the only thing the researcher can do is to get into contact with, get inside, enough business organizations to determine how they do evaluate these factors, how they assign them

weights. Now, if we raise the question—is this type of decision-making a researchable problem?—then we have to face this issue: is it possible to test out formulae of this kind by a series of interviews with financial executives who have faced the problem, and so reach conclusions as to patterns of weighting? This is an important type of decision and a rather general one. Is this a researchable problem in the decision-making category?

MR. COLLYER: I'll stick my neck out on that one. This is researchable. Any number of companies have had to make that decision, and I'm sure that the people who have shared the responsibility would be perfectly willing to sit down with a researcher and tell him exactly how they arrived at the conclusions that were reached.

MR. SIMON: I wonder about that. If you went at this thing really seriously, you could not avoid asking yourself: was this decision really reached on the basis of some identifiable "weighting" of identifiable factors? Or was the decision-making process really of a different kind? Suppose we end up with that familiar checklist of pros and cons that emerges so often in administrative choices. Have we been analyzing the process in the right terms? Do we have the right picture of what takes place, when we say that people make decisions by giving "weights" to the pros and cons? I will conjecture that what we will discover in research on this problem is that the mental processes involved are quite different from what we have supposed—that they aren't connected with the assignment of quantitative values in some kind of mathematical assessment.

MR. HAZARD: I think it would be important to know that. That discovery alone would justify the research. It may well be that a lot of the things management does fall into that non-quantitative category. It reminds me of the

story of how they are alleged to weigh hogs in Texas. As I heard it, they get a long board and they balance it on a log. Then they put the hog on one end of the board and they look around until they find a stone that they can put on the other end of the board to balance the hog. Then they guess the weight of the stone. In this financing problem, you may discover that after all the financial analysts and economists and statisticians and cost experts have done their work, the decision comes out of the finger tips—qualitative, perhaps indescribable, judgment by the president or the chairman of the board.

RESEARCHING MOTIVATIONS

MR. TEELE: The observation was made a moment ago that research in decision making ought to focus on current decisions at the very time when they are being made, rather than conduct a kind of post mortem on past decisions. As a practical matter, how do you do that? My impression is that most decisions of importance, at least in a large enterprise, begin in ten, fifteen, twenty different places. A lot of people are saying, "Well, now, if so-and-so should be done later, what would that involve now?" They're writing memos about it. They're thinking hard and talking their thoughts out with each other. Frequently when you try to cross-examine executives you find that they can't even tell who was involved in a specific decision, much less describe the reasons each person had for thinking the way he did.

I'm sure that if these things are researchable they require a kind of research that is very expensive in time and money. I'm sure that most of the short-cuts that might be dreamed up would give approximations so rough as to be of doubtful value. We've tried, for example, in a current research project to get at the extent to which tax considerations influ-

ence investment decisions on the part of wealthy individuals. We found we could do this only by prolonged depth interviews of a highly sophisticated and devious sort in which the one thing that was never mentioned was the question in which we were really interested. The minute the word "tax" was introduced we began to get answers that reflected what those interviewed thought the answers ought to be rather than what they really were. But by undertaking interviews that might run four or five hours in which we carefully avoided any appearance of interest in the subject that was at the heart of the research, we could make some guesses as to how tax considerations actually did influence investment decisions. I think these questions are researchable. But I am certain that they cost, in time and money, several times as much as most people think.

MR. WHITE: Our experience certainly supports that point. Let me give you one example. A small but growing company in Texas was faced with the decision of whether to expand to a national scale. Many people, who later turned out to have been right in their judgment, thought it would be disastrous for the company to undertake this expansion at that particular time. We spent perhaps a month and a half on that research assignment and did the best we could to discover how the decision actually was made. It was a fascinating case, but we ended with a horrible feeling that we hadn't even begun to get near the real considerations and the real weighting of those considerations that went into the decision. We had a suspicion that one of the important hidden reasons for the decision had nothing to do with the economics of the company's position and outlook, but was simply the desire on the part of some of the officers to have an excuse for putting up a nice new office building. But you can't jump to conclusions like that without a lot of hard dig-

ging. I know that while we got an adequate story in a month and a half, it would have taken perhaps a year to get all the facts and get them properly organized and assessed.

MR. BACH: The group here certainly shows no signs of running out of topics that need to be researched. It is obvious that the day's discussion has developed an impressive list of subjects of fundamental importance where our current knowledge falls far short of what we would like to have in order to handle our management problems. Undoubtedly the list is still vastly incomplete. But all this talk of decision making, execution, and feed-back reminds me that at the start of this morning's discussion I announced a policy decision that we would end this round table—at least in its more formal aspects—at four o'clock. That time is here and I now propose to put that decision into effect.

Rather than attempt to summarize all of the extremely varied discussion we have had today, I am going to suggest that some of us here at Carnegie think over the points covered in this conversation and undertake to present them in summary form as a conclusion to the report of this meeting.

Before we break up, I want to express the pleasure that all of us here at Carnegie have had in talking with you gentlemen about some problems whose solution is critical for the health of this school in the years ahead. We are grateful that you found it possible to take this time from your crowded schedules to be with us.

Some Tentative Conclusions

The round table was organized with several critical questions in mind.

Are business leaders really interested in basic research in administration?

Do they think fundamental research in this area is of such importance that a considerable share of the research facilities of schools of administration should be assigned to it?

What problems in administration are of greatest concern to business leaders?

What difficulties should be anticipated in trying to research these problems?

What techniques of research seem to be most promising?

How can business firms and universities develop more effective research relationships?

We hoped that if a group of outstanding business leaders could sit down for a day to talk about these issues with some university administrators and researchers, the discussion would clarify the problems and suggest ways and means of dealing with them. We anticipated neither definitive answers nor unanimity of opinion.

This is substantially what happened. There was some agreement, some disagreement, some understanding, some misunderstanding. In reviewing the day's discussion we were impressed above all by the firmness with which the business executives urged the importance of fundamental research, in contrast to surveys and evaluations of current practices.

A number of tentative conclusions were in process of for-

mulation during the day. These conclusions can be summarized on two levels: in terms of specific problems and methods; in terms of rather general ideas of broad application. On both levels and however tentative, the conclusions appear to have significance for both industry and schools in guiding their thinking about how to organize, focus, and direct research activities so as to make the most effective use of special knowledge and skills.

Research Techniques in the Physical and Social Sciences

The initial suggestion that business management has made relatively limited use of research in the social sciences, in contrast to the dramatic contributions to industrial operation stemming from research in the physical and natural sciences, provoked a varied response. In part, the comments of the participants suggested a twilight zone of confusion about the meaning of the term "research" with its qualifying adjectives "fundamental" and "applied."

Within the specifically defined fields of research problems and methods, there was rather general agreement on three points. First, the research techniques of the physical and natural sciences can be applied in the social sciences, and specifically in the field of business administration, much more generally than has been the case up to this time. This is true particularly for the quantification of measurements and the observation of isolated variables in controlled environments.

Second, potential applications of these techniques in the social science world encounter serious limitations. Difficulties arise because of the greater number of variables involved and the resulting complexity of their interactions. It is not easy in many problem areas to design experiments character-

istic of the world of management. We may be unable, with existing analytical tools, to frame conclusions generally applicable to a broad range of management problems.

Third, the case approach, which has been the characteristic technique in the early stages of researching management problems, has made valuable contributions to knowledge and understanding and has been particularly helpful in identifying problem areas for further investigation. In the hands of careless users, however, it may invite hasty and weakly-supported generalizations not comparable with the "laws" and "principles" of the physical and natural sciences. It must be strengthened by repeated observations of parallel or related behavior patterns and problem areas on the basis of which reliable generalizations can eventually be developed.

Fundamental and Applied Research

Participants in the round table uncovered significant differences of opinion with respect to the distinction between fundamental and applied research. The semantic cloud was dissipated, at least in good part, by exploration of a series of examples. Most of the round-table participants were in agreement that the special skills and facilities of universities and their staffs are used most effectively when assigned to research that studies the concrete and the specific with the objective of formulating generalizations that embrace many related individual situations and cross several fields of learning. This conclusion urged the importance of close association and cooperation between business organizations and universities.

Areas for Research

The round table directed attention to a number of problem areas of urgent concern to management and apparently

adapted to university research. In most of these areas, the management participants indicated their judgment that (1) more knowledge and better understanding would make a significant contribution to management's ability to deal effectively with its assignment; (2) the basic research effort must be on a considerably broader scale than could be attempted by any one business organization; and (3) successful fundamental research would open the door to a multitude of specific applications in individual companies.

Problems Associated with Retirement

We identified a number of problems associated with various aspects of retirement:

1. Mandatory retirement policies versus voluntary or selective policies.
2. Operating criteria under selective retirement programs.
3. Health aspects of retirement.
4. Financing retirement programs.
5. Effects of retirement programs on continuing members of organizations.
6. Appropriate distinctions in retirement programs between workers and management personnel.
7. Retirement programs in relation to labor organizations.

The participants agreed that problems associated with retirement present some of the most serious and potentially dangerous issues facing the next management generation. On one side are projections of a population with steadily increasing life expectancy. On the other are two problem webs. The first is oriented around the current fragmentary and unsatisfactory understanding of the health and financial

aspects of retirement; the second, around growing recognition of the need for maintaining advancement opportunities in vigorous organizations. In the face of our rudimentary understanding of most of the issues, there is a growing compulsion to frame policies and make commitments that may represent dangerous compromises of freedom of action and flexibility in the future.

Perpetuity of Business Organizations

Closely associated were a series of questions about how to identify and foster the factors that underwrite the healthy growth of business firms. Attention was directed to single firms and whole industries that have yielded to stagnation. Bureaucratization as a management disease is not confined to governmental organizations, but creeps with equal stealth and ease into private firms. The round table recommended study of both business and non-business organizations with the objective of isolating forces of growth and degeneration. The discussion recognized both the amorphous character of the research assignment and the need for pushing the investigation into all aspects of organizational behavior in the empirical exploration of hypotheses.

Individualism in Group-Minded Organizations

There is evidence that recent college graduates and middle management personnel have built strongly-rooted value judgments favoring security as contrasted with risk-taking; skills in interpersonal relations within cooperating groups as against independent individualism; known status as against unknown future opportunities. If these observations are sound, what are the implications for management and for the enterprise system? The required research appears to

point in several directions. What motivational sub-structure is being fostered by our educational and business system? How can individuals of outstanding abilities be identified before their driving power is blunted by organizational pressures toward conformity and smooth interaction, and how can they be encouraged to use their talents in the face of possible disruption of established organizations and established operational procedures? How can top management be encouraged to plan and work toward objectives of growth rather than conservation? Is it true that monetary return can no longer be regarded as a prime motivation for middle and top management? If it is true, what motivation pattern is substituted, and how can it be used to stimulate personnel toward targets of growth, expansion, and innovation? These are a few of the questions awaiting fundamental research—the bases of individual and group psychology, organizational behavior patterns, the formation and acceptance of value systems, the role of education as a preparation for business management, and a multitude of related problem areas.

Finding and Developing Potential Executives

The growth of large organizations has inevitably encouraged specialization in management personnel. This is clearly advantageous in the middle management area. Yet from the ranks of middle management must come the top management of the next business generation. How can men of great promise be identified early in their business careers and exposed to wider administrative horizons, either through special educational programs or diversity of work experience? The round table was deeply concerned with problems of both selection and training. It urged research that will help

industry to identify and develop management personnel, recognizing the great breadth of understanding needed by tomorrow's top administrators.

The Decision-Making Process

In considering management's primary responsibility for making decisions, the round-table participants encouraged the integrated study of the decision-making process, though with some doubts as to the researchability of the problem. They recommended application of the case study technique, with the objective of observing and understanding the organization at work in its total assignment of making and executing decisions. It was noted, however, that fundamental research in this area will be expensive, time-consuming, and very difficult. The suggestion was offered that the research include the scope of management's decision-making freedom and the extent to which the economic and social environment restricts many management decisions.

Management-Labor Relations

Problems in the field of management-labor relations certainly need no endorsement as critical in the American economy today. The round table observed, however, that some current research was out of focus, while other studies were too narrowly conceived. Is there a substitute for the strike as the ultimate bargaining weapon other than government intervention through some form of compulsory arbitration? In broader terms, is the democratic decision-making process to which we have committed ourselves in the political area workable in the economic area? Issues of motivation are again prominent. Of equal importance are procedures that encourage agreement rather than exacerbate differences and invite economic disruption and work stoppage. In both

fields, research opportunities and needs are without definable limits.

Other Economic Problems

As might have been expected, in the course of the discussion of areas for research the round table returned frequently to the economic problems that so profoundly affect management's conduct of its affairs. No effort was made to compile an exhaustive checklist for researchers, but the following issues were identified as of outstanding importance:

1. The modification and control of business cycle fluctuations.
2. Single-company policies in adjusting to cyclical fluctuations and contributing to their abatement.
3. Implications of continuing creeping inflation, including advantages and dangers of a steadily rising price level.
4. Productivity measures as a factor in wage determination.
5. Desirability of translating productivity gains into wage advances rather than price reductions.
6. Improvement of long-range forecasting techniques.

Organization Structure

The round table recognized that a beginning has been made, both by individual companies and university and other research groups, at exploring organization structures. The number of cases studied carefully is so limited, however, that general conclusions have been of dubious value. The behavior patterns of varying organization structures in

different industrial environments and in firms of different sizes constitute an important field for research.

Organizing and Publishing "Hidden" Research

Considerable attention was paid to the research activities of individual companies. It was noted that part of this research may be of broad application and that little of it now finds publication outlets.

Problems in Doing Research

A number of problems likely to be encountered in carrying on research in management were touched on during the day's discussion. In many of the areas singled out as of great urgency for exploration and study, it was observed that recorded research has done no more than scratch the surface. The repetitive study of all aspects of a complex situation as a basis for framing broadly applicable generalizations is an outstanding requirement facing university researchers. In some potential study areas, we still must deal with the problem of inventing research techniques that will permit a first approach toward the accumulation of facts.

In almost all fields there are important issues to be worked out cooperatively by business and the schools in learning how to work together effectively. Much of the research activity undertaken by individuals or teams from the universities inevitably involves observation of business organizations in their daily operation. How can outside researchers be introduced into business organizations without disrupting their affairs, on the one hand, and without creating an artificial environment or special constraints, on the other? Can the research team collecting information attain the anonymity of a piece of furniture so that it can observe a business

firm's life process objectively without influencing that process or shifting it from its established patterns and routines?



A good conference does not end when it is adjourned. Its values do not begin to be realized until the conferees return to their jobs and try to apply to their daily problems the proposals they have discussed and the ideas they have acquired.

Quite properly, the round table raised many questions and gave few answers. We were impressed, however, with the vigor with which the business executives urged the schools to do the job they are particularly well equipped to do—the job of fundamental research. The purpose of the conference was to stimulate a re-thinking of research objectives, methods, and responsibilities throughout a wide group in business and academic organizations concerned with problems of industrial administration. We hope that this volume will help to carry the stimulation of the discussion to many who could not participate directly. Underlying the responsibilities for administering and for teaching how to administer must always be the ultimate responsibility for continuing to learn.

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